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Psychological Bulletin

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Psychological Bulletin

THE USE AND MISUSE OF THE CHI-SQUARE TEST

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AND

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It has become increasingly apparent over a period of several years that psychologists, taken in the aggregate, employ the chi-square test incorrectly. The number of applications of the test does not seem to be increasing, but the number of misapplications does. This paper has been prepared in hopes of counteracting the trend. Its specific aims are to show the weaknesses in various applications that have been made and to set forth clearly the circumstances under which χ^2 can be legitimately applied in testing different hypotheses.

To confirm a general impression that the number of misuses of χ^2 has become surprisingly large, a careful survey was made of all papers published in the *Journal of Experimental Psychology*¹ during the three years 1944, 1945, and 1946. Fourteen papers were found which contained one or more applications of the chi-square test. The applications in only three of these papers (1, 15, 18) were judged to be acceptable. In one other paper (21), the several applications could be called "correct in principle" but they involved extremely small theoretical frequencies. In nine of the fourteen papers (2, 4, 10, 16, 17, 20, 28, 29, 30) the applications were clearly unwarranted. In the remaining case,² it was not possible to determine what had been done; and the author, when questioned twice by letter, did not choose to reply.

The principal sources of error (or inaccuracy) in the fourteen papers

¹ The choice of this particular journal resulted from a belief that the psychologists who publish in it are probably better versed, on the average, in statistical methodologies than are those publishing in other journals. No criticism of the journal nor of individual authors nor of experimental findings is intended. The sole purpose is to illustrate correct and incorrect applications of the chi-square test.

² This paper, by Pronko (27), fails to provide the reader with any basis whatever for forming an independent judgment relative to the correctness or incorrectness of the two applications of χ^2 which were made. In this respect it is a good example of the current trend in papers published in psychological journals to reduce explanations of methods of analyzing data to a point where they are quite unintelligible.

just referred to, as well as in papers published in other journals, are as follows:

1. Lack of independence among the single events or measures³
2. Small theoretical frequencies
3. Neglect of frequencies of non-occurrence
4. Failure to equalize ΣF_o (the sum of the observed frequencies) and ΣF_t (the sum of the theoretical frequencies)
5. Indeterminate theoretical frequencies
6. Incorrect or questionable categorizing
7. Use of non-frequency data
8. Incorrect determination of the number of degrees of freedom
9. Incorrect computations (including a failure to weight by N when proportions instead of frequencies are used in the calculations)

These errors will be explained in detail and illustrated with examples taken for the most part from books and published papers.

It is not surprising that errors of the types listed are frequently made; several of the standard texts to which psychologists turn for statistical guidance contain faulty illustrations. For example, Peters and Van Voorhis (26) make four applications of χ^2 , only one of which is without flaws; and in one of the applications made by Guilford (13, p. 91), there is a failure to equalize ΣF_o and ΣF_t and to calculate the number of degrees of freedom correctly.

A single application made by Peters and Van Voorhis contains the first four errors in the above list. Table I is based on their Table Table XXXV (26, p. 411). Twelve dice were thrown fourteen times, and a record was kept of the number of aces appearing at each throw. The observed frequencies F_o are entered in the second column of the table. A value of χ^2 , given in the last column, was calculated for each of the fourteen throws. A composite value of χ^2 was obtained by summing the separate values. The first of the four errors in this application is that the observed frequencies lack independence. They lack independence because the same twelve dice were thrown each time. This means that, when the frequencies are grouped, it is impossible to take into account the effects of individual differences in the dice and possible compensating effects from one die to another. As a consequence, no statements can be made about the behavior of an individual die, nor

³ The term independence, as here used, has reference to individual or single events. In contrast, the hypothesis of independence that is tested by means of χ^2 specifies a lack of relationship (that is, an absence of interaction) between the variates represented in a contingency table. The events that occur to yield the frequencies of a contingency table must be mutually independent even though the variates are related.

is it possible to generalize the findings to any population of dice from which the twelve can be considered a sample. Therefore, only hypotheses which relate specifically to the twelve dice as a group can be tested. More will be said later about this kind of error.

The second flaw in the application comes from using theoretical frequencies of 2. These values are too low to yield a quantity whose

TABLE I
APPLICATION OF THE CHI-SQUARE TEST BY PETERS AND VAN VOORHIS (26)

Throw	F_o	F_t	$(F_o - F_t)^2$	$\frac{(F_o - F_t)^2}{F_t}$
1	1	2	1	0.5
2	3	2	1	0.5
3	2	2	0	0.0
4	3	2	1	0.5
5	1	2	1	0.5
6	4	2	4	2.0
7	2	2	0	0.0
8	4	2	4	2.0
9	1	2	1	0.5
10	0	2	4	2.0
11	3	2	1	0.5
12	2	2	0	0.0
13	3	2	1	0.5
14	1	2	1	0.5
Σ	30	28		10

distribution approximates the χ^2 distribution. The third mistake is the failure to equalize ΣF_o and ΣF_t , which are shown in the table as 30 and 28 respectively. This mistake is related to a fourth one—a failure to take account of the frequencies of non-occurrence of aces. Any of the four errors is sufficient to invalidate this use of the χ^2 test.⁴

⁴ If the observed and theoretical frequencies of non-occurrence of aces had been used in the calculations, the composite value of χ^2 would have been 12 instead of 10. This difference happens not to be large. But in another illustration used by Peters and Van Voorhis (26, Table XXXVI, p. 414), the difference is large. The value of 14.52 is given in the text. When χ^2 is correctly computed by taking account of the frequencies of non-occurrence, the resulting value is 29.72. The number of degrees of freedom remains the same, but the calculated value of χ^2 is more than doubled.

FUNDAMENTAL THEORY

The two most basic requirements in any application of the chi-square test are (a) independence among the separate measures and (b) theoretical frequencies of reasonable size. These requirements can be shown in an elementary way by examining a two-category distribution of measures. But first, an unequivocal definition of χ^2 is needed.

If z is a normal deviate in standard form defined in relation to population parameters m and σ , then

$$z = \frac{X - m}{\sigma}$$

and

$$\chi^2 = z^2 = \frac{(X - m)^2}{\sigma^2}, \text{ with } df = 1. \quad [1]^*$$

Chi-square with 1 degree of freedom is thus defined as the square of a deviation from the population mean divided by the population variance.

If there are r independent measures of the variate X , there will be r independent values of z , and the resulting formula for χ^2 is

$$\chi^2 = \sum_{i=1}^r z_i^2 = \sum \frac{(X_i - m)^2}{\sigma^2}, \text{ with } df = r. \quad [2]$$

Values of χ^2 may range from 0 to ∞ , and they have frequency distributions which depend upon the value of r . The distribution function of χ^2 , in general form, will be given later. It will then be made clear that the chi-square tests of independence and goodness of fit can be applied unequivocally only to frequency data (or to proportions derived from frequency data). For the present, the plausibility of the two basic requirements stated above will be shown through an examination of the two-category case.

The two-category case. Consider a population of N independent events (things; measures), each of which may fall either into category A or into category B. It is assumed that these categories are clearly defined before samples are drawn and that the category in which a given event falls can be unequivocally determined. A sample is drawn from the population and the sample data are to be employed in determining whether or not a certain hypothesis regarding the proportion of cases in each category is tenable. If p is the expected (theoretical) proportion

* Equation [1] verifies the statement that the square root of χ^2 with $df = 1$ is distributed as z (or Student's t) with $df = \infty$.

for category A and q the expected proportion for category B, it follows that

$$p + q = 1.$$

The probability $P(n)$ that n of the N events will fall into category A is given by the binomial distribution function

$$P(n) = \frac{N!}{n!(N-n)!} p^n q^{N-n}, \quad [3]$$

which is the general expression for obtaining the successive terms arising from the expansion of the binomial $(p+q)^N$. The limiting form of equation [3], as N becomes indefinitely large, is a normal distribution function having a mean of Np and a variance of Npq . In symbols,

$$\lim_{N \rightarrow \infty} P(n) = \frac{1}{\sqrt{2\pi} \sqrt{Npq}} e^{-(n-Np)^2/2Npq}. \quad [4]^*$$

Np is the *population* mean of category A. It is the expected value of n , that is, the theoretical frequency to be associated with category A. Nq is the corresponding theoretical frequency to be associated with category B. Npq is the *population* variance. As stated in equation [1], the square of a deviation from the population mean divided by the population variance is distributed as χ^2 with 1 *df*. Thus, from equation [4], it is seen that the quantity

$$\chi^2 = \frac{(n - Np)^2}{Npq} \quad [5]$$

would be distributed exactly as χ^2 with 1 *df*, provided that N is indefinitely large. In the two-category case, equation [5] may be employed to calculate an approximate value of χ^2 . It should be noted that both p and q appear in the denominator of the right-hand term. No restriction is placed during the calculation; the equation gives an approximate solution for any value of N .

The formula that is commonly used in the two-category case to obtain a value of χ^2 is

$$\chi'^2 = \frac{(n - Np)^2}{Np} + \frac{(N - n - Nq)^2}{Nq}, \quad [6]$$

where Np and Nq are theoretical frequencies and n and $(N-n)$ are the

* If the investigator is concerned with the probability $P(N-n)$ that $(N-n)$ events will fall into category B, the limiting form of equation [3] would be written

$$\lim_{N \rightarrow \infty} P(N-n) = \frac{1}{\sqrt{2\pi} \sqrt{Npq}} e^{-(N-n-Nq)^2/2Npq}. \quad [4a]$$

corresponding observed frequencies. The prime symbol is placed on χ^2 in [6] to distinguish between χ^2 as *defined* by equation [5] and χ^2 as ordinarily *calculated* with formula [6]. When [6] is used, the number of df is 1 less than the number of categories because one restriction ($\sum F_i = N$) is imposed on the theoretical frequencies. Therefore, the number of df for [6] is 1, just as it is for equation [5].

It can readily be shown that χ^2 and χ'^2 are identical quantities. It is for this reason, and this reason alone, that formula [6] may be used in obtaining an estimate of χ^2 in the two-category case.⁷

The foregoing discussion reveals the two limitations that hold in any application of the chi-square test. The first limitation is that χ^2 is correctly used only if the N events or measures are independent. Equations [3] and [4] are valid statements only when independence exists. The second basic limitation relates to the size of the theoretical frequencies. If Np (or Nq) remains small as N becomes large, the limiting form of the binomial distribution function is not a normal distribution, as was assumed in writing equation [4]. If, for any reason, either Np or Nq is small, the limiting form of equation [3] is the Poisson distribution function. Under such circumstances, the quantity on the right of equation [5] would not be the square of a normal deviate divided by the population variance and, consequently, would not be distributed as χ^2 with 1 df . *normal approx. binomial*

It should be emphasized that the categories are assumed to be designated in the population before the individual sample is drawn. It should also be emphasized that the equating of the sums of observed and theoretical frequencies *and* the use of the frequency of non-occurrence (in this case, the frequency with which measures fall in category B) are necessary to establish the identity between the quantities defined in equation [5] and equation [6].

⁷ If observed and theoretical proportions are used in calculating values of χ^2 , equations [5] and [6] become

$$\chi^2 = N \frac{(p_o - p_i)^2}{p_i q_i} \quad [5a]$$

or

$$\chi^2 = N \frac{(q_o - q_i)^2}{p_i q_i}; \quad [5b]$$

and

$$\chi^2 = N \frac{(p_o - p_i)^2}{p_i} + N \frac{(q_o - q_i)^2}{q_i}. \quad [6a]$$

In these equations, N is the total number of cases while p_o and q_o are the observed proportions and p_i and q_i are the theoretical proportions for categories A and B, respectively. The equations may be derived from [5] and [6], and they reveal that if proportions are used instead of frequencies, the values calculated from the proportions must be multiplied by N .

The more general case. It is common to have frequency data that fall into several categories instead of just two. This fact requires an extension of the ideas discussed in the two-category case to encompass any number of categories. The basic features of this extension will now be presented. Actually, no new ideas enter into the development. The proof is mathematically more complex, but the underlying ideas are the same.

Consider a population of N independent events, with k possible outcomes, $v_1, v_2, v_3, \dots, v_k$. Assume that, in the population,

- v_1 occurs with a probability of p_1
- v_2 occurs with a probability of p_2
- v_3 occurs with a probability of p_3
- ⋮
- v_k occurs with a probability of p_k

The joint probability $P(n_i)$ that out of N events exactly n_1 will fall in category v_1 , n_2 will fall in category v_2 , n_3 in v_3 , and \dots n_k in v_k , is given by the multinomial distribution function

$$P(n_i) = \frac{N!}{n_1! n_2! n_3! \dots n_k!} p_1^{n_1} p_2^{n_2} p_3^{n_3} \dots p_k^{n_k}, \quad [7]$$

where

$$\sum_{i=1}^k n_i = N.$$

This is the fundamental expression from which the distribution function of χ^2 is derived. It confirms the statement that the measures (frequencies) in the various cells (categories, classes, etc.) of a multidimensional table must be mutually independent to enable a legitimate application of χ^2 in testing any hypothesis concerning the table. And because equation [7] is written in terms of the frequencies $n_1, n_2, n_3, \dots, n_k$, the chi-square tests of independence and goodness of fit, based as they are on a distribution function derived from [7], may be used unequivocally only in relation to frequency data.⁸

The distribution function of χ^2 , here symbolized by $g_r(\chi^2)$, may be written

$$g_r(\chi^2) = C_r (\chi^2)^{(r+2)/2} e^{-(\chi^2/2)}, \quad [8]$$

where C_r , a coefficient which changes with r , is given by

⁸ This statement limiting the use of χ^2 to frequency data is not meant to exclude certain special applications such as finding confidence limits for a population variance from a known sample variance or testing several sample variances for homogeneity. These special applications are mentioned again toward the end of the paper.

$$C_r = \frac{1}{2^{(r/2)} \Gamma(r/2)}. \quad [9]^*$$

It should be emphasized that equation [8] is an exact distribution function for the quantity defined in equations [2] and [5] but only an approximation of the distribution of the quantity defined by equation [7]. Its use in relation to equation [7] requires three separate approximations, each of which assumes a theoretical frequency of reasonable size. The three approximations are:

1. Replacing each of the factorials in equation [7] by its Stirling approximation.
2. Taking a step similar to the one whereby $(1+[X/n])^n$ is replaced by e^X when n is large.
3. Substituting a continuous integral for a summation of discrete quantities.

All of these approximations are quite acceptable and lead to inconsequential errors so long as Np is reasonably large. This reaffirms the fundamental requirement that *the theoretical frequencies must not be small*, if any calculated value of χ^2 is to be distributed as χ^2 .¹⁰

General computational formula. The formula that is commonly employed in calculating values of χ^2 is

$$\checkmark \quad \chi^2 = \sum \frac{(F_o - F_t)^2}{F_t}, \quad [10]$$

* This expression for C_r contains the gamma function $\Gamma(r/2)$. A gamma function is a function which reduces to a factorial whenever the argument is an integer. In the general case, $(n-1)! = \Gamma(n)$. Equation [9] may be written

$$C_r = \frac{1}{2^{(r/2)} \left(\frac{r-2}{2}\right)!}. \quad [9a]$$

Whenever r is an even number, the factorial in [9a] is an integral number, and its value along with the value of C_r , can be determined in a straight forward manner. On the other hand, if r is an odd number, the "factorial" is fractional, and its value must be determined either by referring to a table of the gamma function or by using the equation

$$\left(\frac{r-2}{2}\right)! = \left(\frac{r-2}{2}\right) \left(\frac{r-4}{2}\right) \left(\frac{r-6}{2}\right) \cdots \left(\frac{r-[r-1]}{2}\right) (\sqrt{\pi}).$$

A reader who is interested in plots of the χ^2 distribution function for various values of r is referred to Lewis (19).

¹⁰ A derivation of equation [8] from equation [7] is presented in considerable detail by Greenhood (11) and can be followed by persons familiar with advanced calculus. Greenhood's development, which is more complete but similar to one given by Fry (8), indicates clearly the limitations which hold in applications of the chi-square test to frequency data. It is not necessary, of course, to derive equation [8] from the multinomial distribution function; it may be derived directly from the joint normal distribution function in n variables.

where F_o and F_t , as usual, are observed and theoretical frequencies and the summation extends over all cells (categories) of the table. The equation holds for any number of categories and reduces to the form of [6] when the number of categories is 2. It serves as a constant reminder that the chi-square tests of independence and goodness of fit can be applied unequivocally only to frequency data.

APPLICATIONS: I. THE GOODNESS OF FIT OF DISTRIBUTION FUNCTIONS

One of the commonest applications of the chi-square test is in evaluating the hypothesis that a set of frequency data can be satisfactorily represented by some specified distribution function. It makes little difference what the function is, so long as its fundamental properties are known. The goodness of fit of binomial, Poisson, and normal distribution functions is often tested.

Two-category case. The correct use of χ^2 in connection with a symmetrical binomial distribution function (where $p=q$) can be illustrated with data obtained in a coin-guessing "experiment." A coin was tossed, and 96 students of elementary psychology each guessed whether the coin came up "heads" or "tails." The hypothesis to be tested is that the guess of each student, like the fall of the coin itself, was a purely chance occurrence and that each student was as likely to say heads as to say tails. The results are shown in Table II.

TABLE II
DATA FROM A COIN-GUESSING EXPERIMENT

H_o (= F_o for heads)	68
H_t (= F_t for heads)	48
T_o (= F_o for tails)	28
T_t (= F_t for tails)	48

As seen in the first and third rows, 68 students guessed heads and 28 guessed tails. The theoretical frequencies are 48 and 48. The value of χ^2 is obtained as follows:

$$\chi^2 = \frac{(H_o - H_t)^2}{H_t} + \frac{(T_o - T_t)^2}{T_t} = \frac{(68 - 48)^2}{48} + \frac{(28 - 48)^2}{48} = 16.67.^{11}$$

With 1 df , this value is significant at better than the 0.1% level of confidence;

¹¹ Two alternative formulas that may be used in the two-category case when $p=q$ are as follows:

$$\chi^2 = (H_o - H_t)^2(2/H_t);$$

$$\chi^2 = \frac{(H_o - T_o)^2}{H_o + T_o}.$$

These formulas are the exact equivalents of the one used above.

so it must be concluded that the guesses of the 96 students were somehow biased in favor of heads.

Additive property of χ^2 , illustrated with two-category data. A fundamental property of χ^2 is indicated by the rule which states that the sum of any number of separate and independent values of χ^2 is distributed as χ^2 , the number of df being the sum of the separate df 's. The rule will now be applied in relation to coin-guessing data. Two applications will be made, the first of which is incorrect. *This incorrect application is purposely included as a means of showing how it differs from a correct application and also of revealing the source of many of the errors made by investigators when employing the chi-square test.*

Ninety-six students of elementary psychology were given five successive "trials" in coin guessing. A single coin was tossed five times. After each toss, the 96 students each guessed whether the coin came up "heads" or "tails." Each student wrote his five guesses in order on a sheet of paper. The turn of the coin was never revealed. The results are summarized in Table III. The

TABLE III
DATA FROM A SECOND COIN-GUESSING EXPERIMENT

	Tosses				
	1	2	3	4	5
H_o	68	49	39	54	54
H_t	48	48	48	48	48
T_o	28	47	57	42	42
T_t	48	48	48	48	48
χ^2	16.67	.04	3.37	1.50	1.50
					$\Sigma = 23.08$

frequencies for toss 1 are the same as those given in Table II.

With the ten pairs of observed and theoretical frequencies in Table III (two pairs for each toss), it is possible to test five separate hypotheses—that the guesses of the 96 students on *any one* of the five tosses were chance occurrences and were as apt to be heads as tails. Five values of χ^2 are given in the bottom row of the table. Each of the values was computed with the formula employed with the data of Table II. The number of degrees of freedom is 1 in each case. For 1 df , the value of χ^2 at the 5% level of confidence is 3.841. Four of the five calculated values are less than this and provide no satisfactory basis for rejecting the hypothesis of chance occurrence.

To secure an estimate of χ^2 which makes the probability of rejecting a false hypothesis large, it is desirable, when conditions warrant, to summate separate estimates of χ^2 and obtain a composite estimate. The number of df for the composite estimate is always the sum of the separate df 's. The sum of the five values in Table III is 23.08. The number of df is 5. The hypothesis under test is that the guesses of students on *five successive tosses* of a coin are purely chance occurrences, with the probability of a guess of heads (by any student on any toss) equal to the probability of a guess of tails. The composite value of

23.08 is significant at better than the 0.1% level of confidence, and would warrant a rejection of the hypothesis if the application of the test were correct.

But it is not correct to summate the five values of χ^2 in Table III. The reason is that the responses of the 96 students from one toss to the next cannot be assumed to have been independent. In other words, the tabulated values of H_0 and T_0 from toss to toss are interdependent. Consequently, there is no way of obtaining an unbiased estimate of the theoretical frequency for any toss beyond the first (unless previous guesses are completely ignored). It is unreasonable to assume that a student's knowledge of his guess on one toss did not influence his guess on succeeding tosses. (It will be shown from the data that such an assumption is unsound.) The χ^2 test should never be based on an assumption that is already known to be false. On a single toss, the guess of each student was independent of the guess of the other students; but between tosses, the five guesses of each student were undoubtedly interrelated.

A lack of independence between separate events (measures) is the commonest flaw in the applications of χ^2 that are made by psychologists. Six of the fourteen papers (2, 10, 17, 28, 29, 30) referred to in the opening paragraphs contain applications having this shortcoming.

There are two ways in which the responses of subjects may be interdependent. They may be related from trial to trial, as they were in the coin-guessing illustration, or they may be internally linked within a single trial. Whenever individual subjects each make more than one response per trial, linkages among the measures within the trial must result unless there are no individual differences. Many investigators ignore this restriction and apply the χ^2 test even though the same subjects are used from trial to trial and make several responses on each trial.

The correct use of the additive principle with two-category data. The conditions under which separate values of χ^2 may be legitimately summated can be illustrated with two-category data. Five non-overlapping groups of subjects made a single guess on each of five successive tosses of a coin. The number of subjects per group was 86. Each subject wrote down his guesses in order on a sheet of paper, the sequence of his guesses remaining on the sheet before him. The results for the five groups are summarized in Tables IV (A to E, inclusive). Twenty-five separate values of χ^2 appear in the bottom rows of the five sections of the table. The question to be answered is: *Which of these 25 values may be legitimately summated and which may not be?* The five values in any one of the sections cannot be meaningfully added for reasons already given in the discussion of the data in Table III. However, the five values in the five sections for any single toss may be summated to yield a single composite value having 5 *df*. For example, a composite value may be obtained for the third toss and may be used to test the hypothesis that, on the third toss of the coin, the guesses of the members of the five groups were random occurrences with the probability of a heads guess equalling that of a tails guess. Neither the presence nor the absence of individual biases would nullify a meaningful test of this hypothesis. If the hypothesis could not be retained, it would be correct to conclude that the guesses were not chance occurrences and were perhaps influenced by what had gone before. The difference between this case and the one illustrated in Table III is clear-cut. The data in Table III are for five successive guesses by one group, and the successive theoretical probabilities cannot be established

TABLE IV
DATA FROM A THIRD COIN-GUESSING EXPERIMENT

	Tosses				
	1	2	3	4	5
<i>A. Group 1</i>					
H_o	63	56	36	45	41
H_t	43	43	43	43	43
T_o	23	30	50	41	45
T_t	43	43	43	43	43
χ^2	18.60	7.86	2.28	0.19	0.19
<i>B. Group 2</i>					
H_o	63	56	42	48	45
H_t	43	43	43	43	43
T_o	23	30	44	38	41
T_t	43	43	43	43	43
χ^2	18.60	7.86	0.05	1.16	0.19
<i>C. Group 3</i>					
H_o	65	55	40	48	46
H_t	43	43	43	43	43
T_o	21	31	46	38	40
T_t	43	43	43	43	43
χ^2	22.51	6.70	0.42	1.16	0.42
<i>D. Group 4</i>					
H_o	68	54	38	52	41
H_t	43	43	43	43	43
T_o	18	32	48	34	45
T_t	43	43	43	43	43
χ^2	29.07	5.62	1.16	3.77	0.19
<i>E. Group 5</i>					
H_o	72	57	30	62	38
H_t	43	43	43	43	43
T_o	14	29	56	24	48
T_t	43	43	43	43	43
χ^2	39.12	9.12	7.86	16.79	1.16

without making specific assumptions concerning prior events. In contrast, when the five χ^2 values for a single toss are taken from the five sections of Table IV and summated, the only assumption that is made is that the guesses on that particular toss were in accordance with a theoretical probability for heads of .50.

Composite values of χ^2 for the five tosses are given in Part A, Table V.

TABLE V
VALUES OF CHI-SQUARE BASED UPON DATA FROM TABLE IV

Part A. Composite Values of χ^2		
Toss	χ^2	df
1	127.90	5
2	37.16	5
3	11.77	5
4	23.07	5
5	2.15	5

Part B. Values of χ^2 for Combined Frequencies		
Toss	χ^2	df
1	125.17	1
2	36.92	1
3	7.82	1
4	14.88	1
5	.149	1

The number of *df* in each case is 5. Except for toss 5, the composite values are all significant at far better than the 1% level of confidence. The value for toss 5 falls near the 80% level and lends support for the belief that on a fifth successive toss of a coin, the probability of a heads guess is .50. The values for tosses 1 and 2 show that, on these tosses, the guesses were strongly biased toward heads. Conclusions regarding tosses 3 and 4 should be made in the light of the large contributions made to the composite values for these tosses by the guesses of group 5 alone. As seen in Table IV-E, the χ^2 values for tosses 3 and 4 are 7.86 and 16.79, respectively. With 1 *df* in each case, both values are significant at better than the 1% level. The corresponding values in the other four parts of the table all fall below the 5% level. However, the deviations on tosses 3 and 4 are in the same direction for all five groups, and this fact indicates a definite departure from chance expectations.

It is legitimate to combine for each toss separately the empirical and theoretical frequencies listed in Table IV-A-B-C-D-E, and use the resulting sums to compute values of χ^2 , each with 1 *df*. Values of χ^2 obtained in this way are given in Part B of Table V. As in the case of the composite values, all of the values except the one for toss 5 are significant at better than the 1% level of confidence. Nevertheless, the procedure of combining frequencies is not recommended except where the theoretical frequencies for each of several duplicated experiments are too small to yield satisfactory individual estimates of

χ^2 . Other things equal, the greater the number of degrees of freedom is, the more stable is a value of χ^2 and the greater is the probability of rejecting a false hypothesis.

Five values of χ^2 may be selected from Table IV-A-B-C-D-E and used compositely to test the hypothesis that the guesses of persons on *five successive tosses* of a coin are chance occurrences, with the probability of a heads guess equalling that of a tails guess. The hypothesis is inclusive enough to cover the entire population from which the groups of subjects were randomly selected. To provide for independence between tosses, it is necessary to choose the five values of χ^2 so that there is one value for each toss and so that no two values are based on the guesses of a single group. To this end, numbers from 1 to 5 were assigned to the five sections of the table. The χ^2 value for toss 1 was taken from the section whose number first appeared in a table of random numbers; the χ^2 value for toss 2 from the section whose number next appeared, and so on. The values thus chosen are shown in Table VI. The composite

TABLE VI
COMPOSITE VALUE OF CHI-SQUARE, BASED ON DATA IN TABLE IV

	Tosses				
	1	2	3	4	5
H_0	63	54	30	48	46
H_1	43	43	43	43	43
T_0	23	32	56	38	40
T_1	43	43	43	43	43
χ^2	18.60	5.62	7.86	1.16	0.42
Composite χ^2	$= 33.66$				

value is 33.66, with 5 *df*. It is highly significant and leaves no grounds for believing that the hypothesis is true.

The discussion in the preceding pages on the necessity for independence between measures can perhaps be further clarified through a consideration of coin-tossing. Three different situations will be described to reveal unmistakable differences in hypotheses to be tested and methods of handling data. Suppose, first, that a single penny is selected at random from a large collection of pennies. This penny is tossed successively, say 100 times, and a record is kept of the way it turns. The probability of a head (or a tail) is .50. The χ^2 test may be applied to determine whether or not the empirical results conform to this theoretical probability. If they do conform, it may justifiably be concluded that the penny is "unbiased." The test is an unequivocal one. The extent to which the investigator should generalize to the collection of pennies from which the single penny was chosen is a matter for personal judgment. The χ^2 test, as made, reveals nothing concerning the probability that the selected penny either represents or misrepresents the collection of pennies.

Suppose next that *two* pennies are randomly selected from a collection of pennies and that each penny is tossed 50 times to give a total of 100 tosses.

The frequencies of occurrence of heads and of tails are combined (pooled) for the two coins. It may be assumed that the probability of a head is .50, but the χ^2 test cannot be *meaningfully* applied to test this theoretical probability.¹² The reason is that each penny makes its own unique contribution to the results. If one of them is biased while the other is unbiased, the obtained value of χ^2 could easily be significant and lead to a rejection of the hypothetical probability, even though it is correct for one of the pennies. Furthermore, one penny could be strongly biased for heads and the other equally strongly biased for tails, and the obtained value of χ^2 would turn out to be insignificant. The possible presence of individual idiosyncrasies precludes an unequivocal application of χ^2 . The same thing would be true if five pennies were randomly selected, each one tossed, say 20 times, and the results pooled; or if 10 pennies were selected and each tossed 10 times, or 20 selected and each tossed five times.

Suppose, finally, that 100 pennies are randomly selected from a collection of pennies, that each penny is tossed a single time, and that the number of heads is recorded. It may be assumed that the probability of a turn of heads, in the population from which the pennies are selected, is .50. The fall of each coin is clearly independent of the fall of every other coin. The χ^2 test may be legitimately applied to determine whether or not the observed frequency of heads conforms to the hypothetical frequency. The results of the test can be generalized to the entire collection of pennies. This would hold even though less than 100 pennies were selected, so long as a sufficient number was chosen to provide theoretical frequencies of the occurrence of heads and the occurrence of tails of sufficient magnitude to warrant an application of the χ^2 test. No statements can be made, of course, regarding the tendencies of any individual penny.

The crucial point is that frequencies obtained from individuals, whether pennies or subjects in psychological experiments, should not be pooled if the χ^2 test is to be used, except when it can be shown that there is an absence of biases or idiosyncrasies among them¹³ or when "interaction" effects are specifically under scrutiny. Results on individuals may be combined, but the combining should be done *after* the χ^2 test has been applied to the data on individuals separately. For example, if two pennies are each tossed 50 times, the χ^2 test may be applied to the results for each penny separately, and then the two values of χ^2 may be added to provide a composite value. Similarly, separate values of χ^2 may be obtained from the guesses made by two individuals. The separate values may then be combined to furnish a single composite value. As in the well-known analysis of variance techniques where each source of variability contributes to the total variability, each source of variability should be allowed to make its contribution to the value of χ^2 . Unfortunately, χ^2 procedures provide no way, as analysis of variance techniques do, of introducing *statistical* controls over individual subjects as a source of variability.

¹² The pooling of two or more sets of frequencies to obtain a single value of χ^2 is warranted if the aim is to study the "heterogeneity" or "interaction" aspects of the data. In this connection, see Snedecor's discussion (31, pp. 191-192) of "pooled" and "total" chi-squares.

¹³ If an investigator firmly intends to restrict all generalizations to the group of persons studied—the group considered *in toto*, as a sort of amorphous mass—then the pooling of individual frequencies may be logically defended. In such a situation, the group is analogous to a single individual and must be treated as such.

Therefore, in the use of χ^2 , the control over individuals must be introduced as an intrinsic part of the sampling process.

Multi-category case (single dimension). Frequency data sometimes fall into several categories along a single dimension. If the frequencies from category to category are independent and if some hypothesis regarding their distribution can be meaningfully set up, then the chi-square test may be used to evaluate the hypothesis, provided that the theoretical frequencies for the various categories are of reasonable magnitude. The correct application of the test in such a situation will be illustrated with data obtained in die throwing. A single die was thrown 120 times. There was no reason for believing that any throw was influenced by any other. The results are given in Table VII, where the first

TABLE VII
DATA OBTAINED IN DIE THROWING

Face	F_o	F_t	$\frac{(F_o - F_t)^2}{F_t}$
1	23	20	.45
2	20	20	.00
3	22	20	.20
4	15	20	1.25
5	18	20	.20
6	22	20	.20
Σ	120	120	2.30

column lists the six faces of the die and the second column gives the number of times that each face turned up. The theoretical probability, on each throw, that any specified face of the die would turn up was $1/6$. Consequently, the theoretical frequency of occurrence of each of the six faces was 20, as shown in the third column of the table. A single restriction, the sum of the observed frequencies, was placed in calculating the theoretical frequencies. Consequently, there are 5 df . With 5 df , the obtained value of χ^2 falls near the 80% level of confidence, and there is no basis for rejecting the hypothesis that the fall of the die was, on each throw, a strictly chance occurrence.¹⁴

¹⁴ It would be possible to use the data in Table VII to test six separate hypotheses—that the appearance of each of the six faces was a chance occurrence, with a probability of $1/6$. For example, to test the hypothesis that the appearance of the ace (one-spot) was a chance occurrence, a value of χ^2 would be computed as follows:

$$\chi^2 = \frac{(23 - 20)^2}{20} + \frac{(97 - 100)^2}{100} = .45 + .09 = .54.$$

The number of df is 1. Observe that in the calculation, the frequency of non-occurrence of the ace was taken into account. (The probability of occurrence of the ace was $1/6$ [$=p$] while the probability of its non-occurrence was $5/6$ [$=q$]. This is an example of an asymmetrical binomial.) It is clear that six separate values of χ^2 could be computed

Another illustration of a multi-category frequency distribution (along a single dimension) comes from results on the coin-guessing experiment. A total of 439 subjects each guessed heads or tails on each of five successive tosses of a coin. All but 9 of the subjects were the ones whose guesses were tabulated in Table IV. The χ^2 test will be applied to evaluate the hypothesis that chance factors operated in determining the frequencies of occurrence of the various possible "patterns" of guesses. With five successive tosses and five successive guesses, there were 32 possible patterns, as shown in the first column of Table VIII. If there was no biasing of the guesses (that is, if the guess of every subject on every trial was as likely to be heads as to be tails), then each of the 32 patterns was as probable as any other; and with $N = 439$, the theoretical frequency for each pattern was $1/32 \times 439 = 13.7$, rounded to the first decimal place. This is the value shown in the third column of the table, except where parentheses appear. Note that 32×13.7 does not equal 439 exactly, but equals 438.4. One of the requirements in the application of χ^2 is that $\sum F_o = \sum F_t$. Therefore, six of the theoretical frequencies are given as 13.8. These are the theoretical frequencies corresponding to the six largest observed frequencies. This insures that any slight error that may result from equalizing $\sum F_o$ and $\sum F_t$ operates to make the test more conservative.

In the calculation of χ^2 for Table VIII, a value of $(F_o - F_t)^2/F_t$ was secured for each row. These values are given in the fourth column, and their sum ($= 681.122$) is the desired estimate. The number of df is $(32-1) = 31$. The only restriction placed in figuring the theoretical frequencies was $\sum F_t$, and this meant the loss of a single degree of freedom. Even with 31 df , the value of χ^2 is so large as to leave no basis whatever for retaining the hypothesis that the guesses were chance occurrences.

Normal distribution function. The chi-square test is often used in evaluating the fit of a normal curve to a set of frequency data. Applications of this type are usually correct except for an occasional failure to equalize $\sum F_o$ and $\sum F_t$, a tendency to use some theoretical frequencies that are too small, and, most importantly, an incorrect specification of the number of degrees of freedom. The correct procedure will now be illustrated.

The distribution of the midterm scores of 486 students in a course in elementary psychology is shown in Table IX. The mid-points of class intervals of ten score units are given in the X -column, frequencies in the F_o -column. The mean M of the distribution of scores is 104.0, while the standard deviation is 16.1. There are two methods that can be used in fitting a normal curve to the data (that is, in calculating the theoretical frequencies that correspond to the observed frequencies). One method involves the estimation of areas under segments of the normal curve through the process of multiplying ordinate values by the class interval.¹⁸ This is an approximation procedure. A more exact method (and the one used here) is to obtain the values for the areas from proportions taken from a table of the probability integral.

in the way just indicated. But these values could not then be legitimately summated to yield a composite value of χ^2 with 6 df . They could not be combined because they would lack independence; the frequency of non-occurrence in each calculation would include the frequency of occurrence of the other five faces.

¹⁸ For an example of this method, see Guilford (13, p. 91).

The column in Table IX labeled X' gives the upper limits of the various score categories. Deviate scores and z scores based on the values of X' are

TABLE VIII
ANALYSIS OF COIN-GUESSING RESPONSES OF 439 SUBJECTS*

Patterns	F_o	F_t	$\frac{(F_o - F_t)^2}{F_t}$
H H H H H	25	(13.8)	9.089
H H H H T	12	13.7	0.211
H H H T H	22	13.7	5.028
H H H T T	18	13.7	1.350
H H T H H	29	(13.8)	16.742
H H T H T	96	(13.8)	489.626
H H T T H	22	(13.8)	4.872
H H T T T	14	13.7	0.007
H T H H H	5	13.7	5.525
H T H H T	15	13.7	0.123
H T H T H	12	13.7	0.211
H T H T T	4	13.7	6.868
H T T H H	33	(13.8)	26.713
H T T H T	17	13.7	0.795
H T T T H	12	13.7	0.211
H T T T T	3	13.7	8.357
T H H H H	3	13.7	8.357
T H H H T	7	13.7	3.277
T H H T H	10	13.7	0.999
T H H T T	14	13.7	0.007
T H T H H	5	13.7	5.525
T H T H T	1	13.7	11.773
T H T T H	6	13.7	4.328
T H T T T	0	13.7	13.700
T T H H H	4	13.7	6.868
T T H H T	6	13.7	4.328
T T H T H	25	(13.8)	9.089
T T H T T	7	13.7	3.277
T T T H H	2	13.7	9.992
T T T H T	5	13.7	5.525
T T T T H	2	13.7	9.992
T T T T T	3	13.7	8.357
Σ	439	439.0	$\chi^2 = 681.122$

* All but nine of the subjects are the same as those whose guesses are analyzed in Table IV.

shown in the fourth and fifth columns. Proportions of the total area under the normal curve from $-\infty$ to z are given in the P column. Proportions of the

area in the segments corresponding to the various score intervals are shown in column P' and were obtained by taking the differences between the successive values of P . The theoretical frequencies came from multiplying the values of P' by N which is 486 in this case.

TABLE IX
APPLICATION OF THE CHI-SQUARE TEST IN EVALUATING THE FIT OF A NORMAL
CURVE TO A SET OF FREQUENCY DATA

X	F_o	X'	x ($=X' - M$)	z ($=x/\sigma$)	P	P'	F_t ($=P'N$)	$\frac{(F_o - F_t)^2}{F_t}$
144.5	1				(+ ∞) (1.0000)			
134.5	22	139.5	35.5	2.195	.9859	.0141	6.9	.926
124.5	56	129.5	25.5	1.574	.9422	.0437	21.2	
114.5	112	119.5	15.5	0.953	.8297	.1125	54.7	.031
104.5	111	109.5	5.5	0.342	.6337	.1960	95.3	2.926
94.5	94	99.5	- 4.5	-0.280	.3897	.2440	118.5	.475
84.5	54	89.5	-14.5	-0.891	.1865	.1211	98.8	.233
74.5	27	79.5	-24.5	-1.512	.0654	.0489	23.8	
64.5	7	69.5	-34.5	-2.133	.0165	.0135	6.6	.555
54.5	2	59.5	-44.5	-2.754	.0030	.0030	1.4	
				(- ∞) (.0000)				
Σ	486				1.0000	486.0	5.538	

Because the first and the last two values of F_t are less than 10, they were combined with the adjacent values, as were the corresponding values of F_o . The sum of the last column in the table (-5.538) is the value of χ^2 . Seven differences between F_o and F_t entered into the calculations. The number of degrees of freedom is $7 - 3 = 4$. Three degrees were lost because three restrictions were placed in determining the theoretical frequencies.¹⁶ The restrictions were

¹⁶ Some statistics texts (6, 25, 26) perpetuate the view, erroneously attributed to Pearson (24), that the number of restrictions imposed in fitting a normal curve is 1 or 3, depending upon the hypothesis that the investigator wishes to test. There is only one hypothesis open to test—that the frequency data arose from a normal population. If

the computed values of $\sum F_o$, M , and σ . The hypothesis being tested is that the frequency data arose from a normal population. With 4 df , the probability of obtaining, by chance, a value of χ^2 greater than 5.538 is around .25; so the hypothesis is tenable.

The Poisson distribution function. If the probability of the occurrence of an event is quite small, so that Np remains small even though N is relatively large, the distribution of observed frequencies in samples of size N may be of the Poisson type. The equation for the Poisson distribution may be written

$$P(n) \doteq \frac{m^n}{n!} e^{-m} \quad [11]^{17}$$

where $m = Np$ and e has its conventional meaning. As in equations [3] and [4], the symbol $P(n)$ represents the probability of n occurrences out of N possible occurrences. The symbol \doteq is used in place of the equal sign to indicate that [11] is an approximation formula. The errors introduced by the approximation

TABLE X
ANALYSIS OF FREQUENCIES OF OCCURRENCE OF THE CONSONANT "TH"
IN SAMPLES OF AMERICAN SPEECH

n	F_o	$P(n)$	F_i	$(F_o - F_i)^2$	$\frac{(F_o - F_i)^2}{F_i}$
0	31	.1868	22.42	73.61	3.283
1	31	.3138	37.66	44.35	1.178
2	30	.2636	31.63	2.66	.084
3	11	.1476	17.71	45.02	2.542
4	11	.0620	7.44		
5	3	.0209	2.51		
6	3	.0050	.60	41.22	3.896
>6		.0003	.03		
Σ	120	1.0000	120.00		$\chi^2 = 10.983$

are negligible, provided that N is quite large and provided also that N is very much larger than the largest value of n that may reasonably be expected in random sampling.

The χ^2 test may be applied in relation to the data of Table X. The column labeled F_o gives the number of samples, in a total of 120 short samples of

the mean and standard deviation of the fitted function are estimated from the data, three restrictions are imposed and 3 df are lost. The same texts give a similar misinterpretation of the number of restrictions imposed when χ^2 is applied in testing independence.

¹⁷ A derivation of this formula is given by Lewis (19, pp. 168-169).

American speech, that contained n occurrences of the consonant "th" (as in thin). Each of the 120 samples was 400 sounds in length. As seen from the table, 31 of the samples did not contain any "th" sounds; 31 samples contained one "th" sound each; and so on. As a first step, the data in Table X will be compared with some results obtained by Voelker (33). In a study of over 600,000 sounds occurring in almost 6,000 announcements over the radio, Voelker found the proportion of "th" sounds to be .0065. Each of the 120 samples represented in Table X contained 400 sounds. This made a total of 48,000 sounds. The use of the proportion obtained by Voelker leads to 260 as the predicted, or theoretical, number of "th" sounds among the 48,000. The observed number was 201.¹⁸ The χ^2 test may be applied in evaluating the hypothesis that the sounds in the present over-all sample, were drawn from a general population of American speech sounds which is assumed to be characterized exactly by the value of m obtained by Voelker. The value of χ^2 is computed as follows:

$$\chi^2 = \frac{(201 - 260)^2}{260} + \frac{(47,799 - 47,740)^2}{47,740} = 13.388 + 0.073 = 13.461.$$

Note that the observed and theoretical frequencies of non-occurrence of the "th" enter into the calculation. With 1 df , the obtained value of χ^2 is significant at better than the 0.1% level of confidence; so there is a firm basis for rejecting the hypothesis.

The observed proportion of .0042 ($= 201/48,000$) may be used in testing the hypothesis that the 120 samples were all drawn from the same Poisson distribution. If $p_0 = .0042$ and $N = 400$, then $m = Np = 1.68$, and the equation for the hypothetical distribution function may be written

$$P(n) = \frac{(1.68)^n}{n!} e^{-1.68}.$$

Values of $P(n)$ computed with this formula are given in the third column of Table X, and the corresponding values of F_t are given in the fourth column. As shown in the last column of the table, the value of χ^2 was computed in accordance with equation [10]. The last 4 values of F_t and the last 3 values of F_o were combined to avoid the use of theoretical frequencies of less than 10. The computed value of χ^2 is 10.982. Five differences between F_o and F_t were used in the computations. Two restrictions (N and m)¹⁹ were imposed in calculating values of F_t . This leaves 3 df . With this number of df , the obtained value χ^2 falls at about the 3% level of confidence. The hypothesis may, therefore, be tentatively retained or may be rejected, depending upon the level of confidence that has been prescribed.

APPLICATIONS: II. THE CHI-SQUARE TEST OF INDEPENDENCE

A common application of the χ^2 test enables an examination of the frequencies of a contingency table to determine whether or not the

¹⁸ $\sum nF_o = 0(31) + 1(31) + 2(30) + 3(11) + 4(11) + 5(3) + 6(3) = 201.$

¹⁹ If the value of p had not been estimated from the empirical data, but had been taken from the Voelker study or from some other completely independent source, the only restriction that would have been placed would have been N , and there would have been 4 df instead of 3 df .

two variables or attributes represented in the table are independent. The number of cells in the table may range from four (as in a 2×2 table) to an indefinitely large value. The χ^2 test is perhaps most commonly applied by psychologists in relation to 2×2 tables. The chief weaknesses in such applications are (a) a strong tendency to use excessively small theoretical frequencies and (b) an occasional failure to categorize adequately. The same two weaknesses are apt to occur when the number of categories in either, or both, of the "dimensions" of the table is greater than two.

Illustrations of the Chi-Square Test of Independence

Comparison of coin- and die-coin guessing. To obtain data for illustrating the χ^2 test of independence, 384 students of psychology were each asked to guess heads or tails on five successive tosses of a coin, where the tosses were interspersed among five throws of a die. The die was thrown; a guess was made as to the face that turned up. This guess was written on one edge of a sheet of paper. The edge of the paper was then folded under, to hide the guess. The

TABLE XI
RESULTS FROM COIN- AND DIE-COIN GUESSING EXPERIMENTS

Patterns	F_o			F_t	
	Coin Guessing	Die-Coin Guessing	Totals	Coin Guessing	Die-Coin Guessing
H H H H H	25	29	54	28.8	25.2
H H H H T	12	9	21	11.2	9.8
H H H T H	22	9	31	16.5	14.5
H H H T T	18	5	23	12.3	10.7
	—	—			
	77	52			
H H T H H	29	38	67	35.7	31.3
H H T H T	96	59	155	82.7	72.3
H H T T H	22	24	46	24.5	21.5
H H T T T	14	8	22	11.7	10.3
	—	—			
	161	129			
H T H H H	5	7	12	6.4	5.6
H T H H T	15	9	24	12.8	11.2
H T H T H	12	2	14	7.5	6.5
H T H T T	4	9	13	6.9	6.1
	—	—			
	36	27			

TABLE XI—Continued

Patterns	<i>F_o</i>			<i>F_t</i>	
	Coin Guessing	Die-Coin Guessing	Totals	Coin Guessing	Die-Coin Guessing
H T T H H	33	12	45	24.0	21.0
H T T H T	17	20	37	19.7	17.3
H T T T H	12	3	15	8.0	7.0
H T T T T	3	4	7	3.7	3.3
—	—	—	—	—	—
Sub-totals	(339)	(247)	65 39	—	—
T H H H H	3	3	6	3.2	2.8
T H H H T	7	5	12	6.4	5.6
T H H T H	10	9	19	10.1	8.9
T H H T T	14	3	17	9.1	7.9
—	—	—	34 20	—	—
T H T H H	5	4	9	4.8	4.2
T H T H T	1	8	9	4.8	4.2
T H T T H	6	4	10	5.3	4.7
T H T T T	0	1	1	0.5	0.5
—	—	—	12 17	—	—
T T H H H	4	13	17	9.1	7.9
T T H H T	6	19	25	13.3	11.7
T T H T H	25	37	62	33.1	28.9
T T H T T	7	13	20	10.7	9.3
—	—	—	42 82	—	—
T T T H H	2	4	6	3.2	2.8
T T T H T	5	5	10	5.3	4.7
T T T T H	2	1	3	1.6	1.4
T T T T T	3	8	11	5.9	5.1
—	—	—	12 18	—	—
Sub-totals	(100)	(137)	—	—	—
Totals	439	384	823	438.8	384.2

coin was then tossed, the guess being written down. Again the paper was folded under, to hide the guess. The die was thrown a second time, the guess made, the paper folded under. The coin was then thrown a second time, the guess made, the paper folded under. Each of the five guesses on the coin was preceded by a throw of the die and a guess on its fall. The paper was folded under after each guess on the die and each guess on the coin. Thus, the guesses on the coin were not only separated by guesses on the die, but the sequence of guesses was hidden from view. The subjects were never informed as to how the coin or the die actually fell.

The frequencies of occurrence of the 32 possible patterns of guesses on the five successive tosses of the coin (with guesses on the die ignored) are given in the third column of Table XI. The frequencies in the second column of this table were copied directly from Table VIII and are based on guesses on five successive tosses of a coin alone.²⁰ The subjects for the two conditions of guessing were completely different.

TABLE XII
2×2 CONTINGENCY TABLE BASED UPON THE COIN- AND DIE-COIN
GUESSING EXPERIMENTS

	<i>First Guess</i>		439
	<i>H</i>	<i>T</i>	
Coin Guessing	339 (312.6)	100 (126.4)	
Die-Coin Guessing	247 (273.4)	137 (110.6)	384
	586	237	823

The first 16 patterns listed in Table XI begin with a guess of heads, the last 16 with a guess of tails. A 2×2 contingency table, shown in Table XII, was set up, the division along one "dimension" being between first-guess-heads and first-guess-tails and along the other "dimension" between the two conditions of guessing. The four sub-totals in Table XI constitute the observed frequencies appearing in the four cells of the 2×2 table.

The hypothesis to be tested in this case is that the occurrence of a first guess of heads was independent of the condition under which the guessing was done. If the observed frequencies in Table XII were independent of the conditions of

²⁰ As will be seen, the 32 patterns in Table XI have been divided into groups of four patterns each. The basis for the division will be discussed later. Short horizontal lines divide the corresponding observed frequencies. A numeral is placed to the right of each of these lines. Each numeral is the sum of the observed frequencies for the corresponding group of four patterns. Each is a sub-sum and will be used in Table XIII.

guessing, the probability of a guess of heads was 586/823. The probability of a subject's being in the coin guessing group was 439/823. The joint probability that a subject would be in the coin guessing group and would also guess heads was $439/823 \times 586/823$. The theoretical frequency for the upper left-hand cell of the table was obtained by multiplying this joint probability by 823. The other three theoretical frequencies were automatically determined by this single calculated frequency and by the restrictions of the border sums. The four values of F_t are shown in parentheses in the table. The value of χ^2 was computed as follows:

$$\begin{aligned}\chi^2 &= \frac{(339 - 312.6)^2}{312.6} + \frac{(100 - 126.4)^2}{126.4} + \frac{(247 - 273.4)^2}{273.4} + \frac{(137 - 110.6)^2}{110.6} \\ &= (26.4)^2 \left(\frac{1}{312.6} + \frac{1}{126.4} + \frac{1}{273.4} + \frac{1}{110.6} \right) \\ &= 696.96 (.02381) = 165.946.\end{aligned}$$

The number of df is 1. This follows because there are four cells and because three restrictions were placed in determining the theoretical frequencies. The three restrictions were the total number of subjects (= 823) and two border sums, one for a row and one for a column.²¹ The calculated value of χ^2 is highly significant and leads immediately to a rejection of the hypothesis that the conditions of guessing had no influence on the tendency to guess heads on the first guess.

Table XI may be regarded as a 32×2 contingency table, 32 "patterns" by 2 "conditions of guessing." In order to use the χ^2 test in evaluating the hypothesis that the patterns were independent of the conditions of guessing, the border sums would be used in calculating theoretical frequencies. For example, the two theoretical frequencies for the pattern $H\ H\ H\ H\ H$ are given by the relations: $F_t = (54 \times 439)/823 = 28.8$; and $F_t = 54 - 28.8 = 25.2 = (54 \times 384)/823$. These two values of F_t are shown in the top row of the last two columns of Table XI. The theoretical frequencies for the other 31 patterns were obtained in a similar way and are listed in the table. These frequencies are included to emphasize the fact that the χ^2 test cannot be legitimately applied to this table as it stands. The reason is that 37 of the 64 theoretical frequencies are less than 10 (some of them very much less than 10) and cannot be depended upon to yield quantities distributed as χ^2 . To make a legitimate application of χ^2 in this particular case, it would be necessary to increase the number of subjects to a point where the smallest of the theoretical frequencies was close to 10.

It must now be decided whether or not the observed frequencies in Table XI can be combined so as to permit the use of the χ^2 test in evaluating the hypothesis of non-relationship between the patterning of the guesses and the conditions of guessing. The frequencies have already been combined in a gross way to yield the observed frequencies in Table XII. This division was on the basis of the first guess. Divisions might be made on the basis of the first two guesses or the first three guesses or the first four guesses. It is not possible to use the patterning on all five guesses because the theoretical frequencies become too small, as already seen. It turns out that a division on the basis of the first four

²¹ The sums 439 and 237 (or 384 and 237, or 384 and 586) could have been used instead of 586 and 439. The values of F_t for all but one cell may be obtained by subtraction.

guesses also leads to several theoretical frequencies that are less than 10. Consequently, a division based on the patterning of the first three guesses will be illustrated. In the division that was made, the following rule held: there would be a decreasing number of heads in the pattern and, contrariwise, there would be an increasing number of tails, from the first guess on. The resulting division of the patterns is shown in Table XI and also in Table XIII, the one to be used in applying the χ^2 test.

TABLE XIII
COMBINATION OF THE FREQUENCIES SHOWN IN TABLE XI, BASED ON
THE FIRST THREE GUESSES

<i>Pattern on 1st Three Guesses</i>	<i>Coin Guessing</i>	<i>Die-Coin Guessing</i>	<i>Totals</i>
H H H	77 (68.8)	52 (60.2)	129
H H T	161 (154.7)	129 (135.3)	290
H T H	36 (33.6)	27 (29.4)	63
H T T	65 (55.5)	39 (48.5)	104
T H H	34 (28.8)	20 (25.2)	54
T H T	12 (15.5)	17 (13.5)	29
T T H	42 (66.1)	82 (57.9)	124
T T T	12 (16.0)	18 (14.0)	30
Totals	439	384	823

As seen, there are eight different patterns listed in Table XIII. The observed frequencies for the two conditions of guessing are shown (together with parenthesized theoretical frequencies) in the second and third columns. The hypothesis to be tested is that the patterns of guessing on the first three of five consecutive guesses were independent of the conditions of guessing. The theoretical frequencies were secured in the usual way by employing border sums and the value of $N (= 823)$. The value of χ^2 was computed as follows:

$$\chi^2 = \frac{(77 - 68.8)^2}{68.8} + \frac{(161 - 154.7)^2}{154.7} + \dots + \frac{(82 - 57.9)^2}{57.9} + \frac{(18 - 14.0)^2}{14.0} = 31.162.$$

The number of df is 7. The computed value of χ^2 is significant at far better than the 1% level of confidence and leads to a rejection of the hypothesis. It may be confidently concluded that the patterning of the guesses through the first three guesses was somehow influenced by the conditions under which the guessing was done.

TABLE XIV
5×4 CONTINGENCY TABLE BASED UPON LINDQUIST'S DATA (23)

Scores	Enrollment Groups*				Totals
	A	B	C	D	
1	36 (23.5)	40 (38.4)	20 (28.2)	3 (8.9)	99
2	76 (60.2)	108 (96.8)	59 (72.3)	11 (22.9)	254
3	150 (111.4)	181 (182.5)	111 (133.7)	28 (42.4)	470
4	211 (219.5)	342 (359.6)	285 (263.5)	88 (83.5)	926
5	66 (124.4)	212 (203.8)	172 (149.4)	75 (47.3)	525
Totals	539	883	647	205	2274

* See footnote 22.

The number of df for a value of χ^2 obtained from a contingency table is always the number of cells in the table minus the number of restrictions imposed during the calculation of the theoretical frequencies. In Table XIII, for example, there are 16 cells. Nine restrictions must be imposed in obtaining values of F_i . These restrictions are: 7 of the row sums, 1 of the column sums, and the total number of cases. Thus, $df = 16 - 9 = 7$.

A convenient formula for determining the number of df for a contingency table when the χ^2 test is applied is

$$df = (n_c - 1)(n_r - 1), \quad [12]$$

where n_c and n_r are the number of columns and the number of rows, respectively. There is only one hypothesis to be tested—that the variables are independent in the population from which the samples arise; so the number of df is always given by [12]. (See footnote 16.)

Contingency table with more than two categories in each direction. For the sake of completeness, a contingency table having five categories in one direction

and four in the other will be included. A total of 2,274 eighth-grade pupils, enrolled in 91 different schools, took an English Correctness Test. A summary of the scores obtained by these pupils has been taken from a report by Lindquist (23). The scores were divided into five categories and symbolized by numbers from 1 to 5, as seen in Table XIV. The schools were divided into four enrollment groups, labeled A to D in the table. The observed frequencies in the 20 cells of the table range from 3 to 342. These frequencies would obviously have been different if the enrollment groups had been differently established, and if the scores had been divided into different categories. The enrollment grouping of the schools was that commonly used in the Iowa Every-Pupil Testing Program.²² The division of the scores was made by starting at the bottom and "stepping off" successive standard deviation "distances" (approximately). The distribution of scores was positively skewed; so it was necessary to combine the two upper score categories to provide satisfactorily large frequencies in the top row of cells.

The theoretical frequencies for Table XIV were computed in the usual way when the hypothesis of independence is under test. The computations required the use of three column sums and four row sums, as well as the value of N ($= 2274$). This made a total of 8 restrictions; so the number of $df = 20 - 8 = 12 = (5 - 1)(4 - 1)$. The value of χ^2 was computed as follows:

$$\chi^2 = \frac{(36 - 23.5)^2}{23.5} + \frac{(76 - 60.2)^2}{60.2} + \dots + \frac{(88 - 83.5)^2}{83.5} + \frac{(75 - 47.3)^2}{47.3} = 99.038.$$

With $df = 12$, this value is highly significant and leads to a rejection of the hypothesis that the scores obtained on the test were independent of school size.

A comment should be made concerning the theoretical frequency in the upper right-hand cell of the table. Its value is 8.9. Ordinarily, a value this small should not be used in obtaining an estimate of χ^2 . In this case, however, the other 19 values of F_t are satisfactorily large, and the inclusion of one theoretical frequency that is less than 10 is permissible since an error in a single category will have slight effect on the resulting value of χ^2 . The obtained value of χ^2 is so large that it makes no difference whether or not the small theoretical frequency is included in the calculations. It is only in situations of this general kind that one or two small theoretical frequencies may be retained. When the number of df is less than 4 or 5, and especially when $df = 1$, the use of theoretical frequencies of less than 10 should be strictly avoided.

Use of the Chi-square Test with too Small Theoretical Frequencies

The studies of Lewis and Franklin (21) and Lewis (20). The commonest weakness in applications of the χ^2 test to contingency tables is the use of extremely small theoretical frequencies. This weakness is clearly present in most of the applications made in a paper by Lewis and Franklin (21). The paper is concerned with the Zeigarnik effect (that is, with the relative amounts of recall of interrupted and completed tasks). In one experiment, 12 subjects were each presented with 18 problems, 9 of which were interrupted by the experimenter, the other 9 being completed without interruption. The ratio (RI/RC) of the

²² The enrollment categories were: A, greater than 400; B, 126-400; C, 66-125; D, less than 66.

number of interrupted tasks recalled to the number of completed tasks recalled is given for each subject in the top row of Table XV. In a previous study,

TABLE XV
DATA UPON THE "ZEIGARNIK EFFECT" PRESENTED BY LEWIS (20),
AND LEWIS AND FRANKLIN (21)

Ratio of Recall Scores	Subjects													
	1	2	3	4	5	6	7	8	9	10	11	12	13	14
RI/RC—Group I (Indiv. Work Condition)	1.00	.80	.80	.80	.75	.71	.63	.57	.57	.44	.40	.25		
RI/RC—Group II (Coop. Work Condition)	1.67	1.25	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	.80	.75	.60	.14

Lewis (20) had employed a cooperative work situation in which a co-worker completed the tasks on which the subject was interrupted. The conditions were otherwise the same as in the later experiment by her and Franklin. Fourteen subjects were used. The *RI/RC* ratio for each subject is shown in the second row of Table XV. The median ratio for Group I was .67. The ratios for the two groups were divided (dichotomized) at this point. The 2×2 contingency table shown as Table XVI was the result. The hypothesis to be tested was that the conditions of the two experiments had no differential effects on the recall of interrupted and completed tasks. The theoretical frequencies are shown in parentheses in the table. The investigators calculated a value of χ^2 in the usual way. Finding it to be approximately 3.8 and to fall near the 5% level of confidence, they were inclined to reject the hypothesis.

Entirely aside from any of the conclusions reached by Lewis and Franklin, it must be firmly stated that all four of the theoretical frequencies in Table XVI, and especially the two that are less than 5, are too small to warrant an application of the χ^2 test. Furthermore, the other applications made in their paper, with one or two possible exceptions, involved theoretical frequencies that should be avoided (theoretical frequencies, for example, of 4.4, 6.0, 7.0, 7.6, etc.).

TABLE XVI
CONTINGENCY TABLE BASED UPON DATA IN TABLE XV

RI/RC	Group		18
	I	II	
Greater than 0.67	6 (8.3)	12 (9.7)	18
Less than 0.67	6 (3.7)	2 (4.3)	8
	12	14	26

Kuenne's study of transposition behavior. An application, which is correct in principle but which must be regarded somewhat unfavorably because of the size of the F_1 's, comes from a paper by Kuenne (18). Kuenne made a study of transposition behavior in four groups of young children, the ages for the groups being 3, 4, 5, and 6 years. Some of the children displayed size transposition, others of them did not. Kuenne realized that, because of the small number of cases in each age group, she could not apply the χ^2 test to the data for the four groups considered separately. Consequently, she combined the results for ages 3 and 4, and those for ages 5 and 6. The children were divided into two categories—those who did and those who did not meet the transposition criterion. The resulting 2×2 contingency table is shown as Table XVII. The hypothesis

TABLE XVII
CONTINGENCY TABLE BASED UPON KUENNE'S (18) TRANPOSITION DATA

Age-Groups	Transposition	Non-Transposition	Total
3-4 years	3 (9.4)	15 (8.6)	18
5-6 years	20 (13.6)	6 (12.4)	26
	23	21	44

to be tested is that the occurrence of transposition behavior was independent of age. Theoretical frequencies were determined by using two of the border sums and the value of N . It will be seen that two of these frequencies are less than 10. Because they are fairly close to 10, many investigators would proceed as Kuenne did, and make a χ^2 test of the hypothesis of independence. In fact, there are textbooks in statistics which place 5 as the minimum value for theoretical frequencies. A value of 5 is believed to be too low. In any event, it is the smallest value that should be used even when there are several other theoretical frequencies that are far greater than 10.

The value of χ^2 computed from Table XVII is 15.434. With 1 df , this value falls close to the 0.1% level. It is only because the value is so large that confidence can be placed in the conclusion that transposition behavior was related to age. In view of the smallness of all four of the theoretical frequencies, very great doubt would have remained if the χ^2 value had fallen at a border-line level of confidence. Whenever small theoretical frequencies enter into calculations of χ^2 , the experimenter has no sound basis either for accepting or rejecting a hypothesis except when the value is quite extreme.

Yates' correction for continuity. No mention has as yet been made of a correction proposed by Yates (30) which reduces the value of χ^2 to compensate for errors which may arise as a result of one of the approximations made in deriving the formula for the χ^2 distribution. It will be recalled that three approximations are made in this derivation. One of the three involves the substitution of an

integral for a summation of discrete quantities. This approximation introduces an error (an error of overestimation) that is of consequence when values of F_t are small. The correction is justified only when the number of df is 1.²³ It provides for the reduction of all differences between observed and theoretical frequencies by 0.5. For example, all of the differences between observed and theoretical frequencies in Table XVII are 6.4. These are reduced to 5.9 if Yates' correction is applied. The calculation, using the correction, would be as follows:

$$x^2 = (5.9)^2 \left(\frac{1}{9.4} + \frac{1}{8.6} + \frac{1}{13.6} + \frac{1}{12.4} \right) = (34.81)(.3768) = 13.1.$$

With 1 df , this value is still highly significant and leads to a rejection of the hypothesis. But the use of Yates' correction does not remove the objection to theoretical frequencies that are less than 10.

Questionable or Incorrect Categorizing

Lewis' (20) study of recall of interrupted and completed tasks. Another weakness which is sometimes present in applications of the χ^2 test to contingency tables is that the categorizing is done on either a questionable or a clearly incorrect basis. An illustration of incorrect categorizing is found in the paper by Lewis (20) discussed above. The RI/RC ratios for 14 subjects were obtained in a "cooperative work experiment." The ratios are the ones given in the second row of Table XV. On the assumption that the recall of interrupted and completed tasks should have been the same, Lewis writes: ". . . we should have an equal number of ratios above 1.00 and below 1.00. . . . The expected distribution of ratios should, therefore, be 7 below 1.00 and 7 at 1.00 or above. The obtained distribution of ratios is 4 below 1.00 and 10 at 1.00 or above." The categorizing is plainly wrong; there is no more reason for placing an obtained ratio of 1.00 in the upper category than for placing it in the lower category. A better procedure would have been to divide the 8 ratios of 1.00 equally between the two categories. This flaw in Lewis' division of the ratios is reminiscent of the belief of many graduate students in psychology that it is quite permissible to set up several different sets of dichotomy lines, compute a value of χ^2 for each set, and finally select the dichotomies that yield a χ^2 value to support the experimenter's own point of view. In any investigation where the χ^2 test is to be applied, the categories must be established in a logically defensible and reliable manner—before the data are collected, if possible.

Anastasi and Foley's (1) study of drawings of normal and abnormal subjects. The whole problem of categorizing may be brought clearly before the reader by taking an illustrative case from a study by Anastasi and Foley (1). These two investigators had each of 340 normal subjects and 340 abnormal subjects draw a picture which depicted danger. The pictures were then divided into the 20 subject-matter categories listed in Table XVIII. The application of the χ^2 test yields a value of 99.603 which, with 19 df , falls far beyond the 0.1% level of confidence. This leads to a rejection of the hypothesis that the subject matter of the drawings was independent of the two "kinds" of subjects.

²³ The correction should not be made if several values of χ^2 are to be summated. The additive principle does not apply to corrected values.

TABLE XVIII

DATA FROM ANASTASI AND FOLEY'S (1) STUDY OF DRAWINGS OF
NORMAL AND ABNORMAL SUBJECTS

Subject-Matter Categories	F_o (Abnormal)	F_o (Normal)	F_t	χ^2
1. Traffic	105	124	114.5	1.576
2. Conventional sign or signal	36	37	36.5	.014
3. Skating, ice	5	10	7.5	1.667
4. Falling	21	17	19.0	.421
5. Drowning, sinking, flood	8	10	9.0	.222
6. Falling objects, explosion	6	6	6.0	.000
7. Arms and explosives	10	22	16.0	4.500
8. War	3	7	5.0	1.600
9. Fire	26	37	31.5	1.921
10. Lightning, electricity	6	15	10.5	3.857
11. Animals	3	16	9.5	8.895
12. Abstract or conventionalized symbolism	3	8	5.5	2.273
13. Fantastic compositions	26	0	13.0	26.000
14. Several discrete objects	9	2	5.5	4.454
15. Scribbling or scrawl	6	0	3.0	6.000
16. Writing only	12	1	6.5	9.308
17. Miscellaneous	23	26	24.5	.184
18. Recognizable object not representing danger	8	1	4.5	5.444
19. Refusal to draw	14	1	7.5	11.267
20. No data for other reasons	10	0	5.0	10.000
	340	340		99.603

Let it again be emphasized that the criticism here, as elsewhere in the paper, is not directed at any of the conclusions reached by the investigators. But the illustration provides a very satisfactory basis for discussing the fundamental problem of categorizing. In the published article, the principles adopted in classifying the pictures are not explicitly stated. Furthermore, evidence is not presented regarding the reliability of the categories. Two generalizations may be offered. The first is that, whenever possible, categories for frequency data should be established on the basis of completely external criteria (for example, criteria that have been used or proposed by some other investigator) and should be set up independently of the data under study. Such a procedure frees a person from any charge of bias and guards against tendencies to juggle data. A second generalization is that information on the reliability of categories should be offered, and this is the case whether or not the categories have stemmed from an independent source.

A study of Table XVIII shows rather quickly that the value of χ^2 for the drawings depicting danger would have been quite different if the categorizing

had been different. For example, the amount 26.000 was contributed to the value of χ^2 by the frequencies of category 13 alone. As seen, this is the category "fantastic compositions." The decision which established this category and the judgments which placed 26 of the drawings of the abnormal subjects in the category and none of the drawings of the normal subjects in it, should have been explicitly justified and a precise statement concerning reliability should have been included. The discrepancy between the frequencies in category 13 (along with the discrepancies in such categories as "refusal to draw" and "no data for other reasons") required that there be discrepancies in one or more of the other categories. Unreliability at one point in a multicelled table automatically produces unreliability elsewhere.

It is well to emphasize, by reiteration, that when the χ^2 test is to be applied to a collection of data, the categories should be established independently of the data and, once established, should never be modified on the basis of the way the data happen to fall. Categories should usually, if not always, be established before the data have been scrutinized.

APPLICATIONS: III. THE GOODNESS OF FIT OF FUNCTIONS IN WHICH FREQUENCY IS THE DEPENDENT VARIABLE

The frequency (or relative frequency) of occurrence of a response is sometimes used as the dependent variable in psychological experiments. For example, in psychophysical investigations based on the method of constant stimuli, the number or proportion of judgments in a given direction serves as the dependent variable, while in studies of the conditioned response, the frequency of occurrence of the CR is often taken as the dependent variable. If a mathematical function is fitted to data of this type, it is sometimes possible to apply the χ^2 test in evaluating the goodness of the fit. However, care must be taken to insure a correct application.

Goodness of fit of the phi-gamma function. It is a fairly common practice among psychophysicists to apply the χ^2 test to the differences between observed and theoretical proportions in order to evaluate the phi-gamma hypothesis—the hypothesis that the observed proportions can be represented satisfactorily by the phi-gamma function.²⁴ The usual procedure in making the test will be illustrated with some weight-lifting data.

As part of a laboratory exercise, a graduate student in an advanced experimental course made 100 judgments on each of nine pairs of weights. The weights, in grams, are listed in the X column of Table XIX. The method of constant stimuli was employed. The 100-gram weight was the standard and was paired 100 times, not only with itself, but with *each* of the other eight ("variable") weights. The standard weight was always lifted first, the variable weight second. Each of the 900 judgments was made in terms of the question: Which weight is the heavier, the first or the second? The second column of the

²⁴ The use of the phi-gamma function in psychophysical research is explained by Guilford (13, Chap. VI).

TABLE XIX
TEST OF GOODNESS OF FIT OF THE PHI-GAMMA FUNCTION TO WEIGHT-LIFTING DATA

X	p_o	γ	$X\gamma$	X^2	γ_t	p_t	(A)		(B)	
							$(p_o - p_t)^2$	$\frac{(p_o - p_t)^2}{p_t q_t}$	p_o	$n_t \frac{(p_o - p_t)^2}{p_t q_t}$
104	.97	1.3299	138.31	10,816	1.246	.960	.000100	.002600	.935	.934
102	.90	0.9062	92.43	10,404	.942	.908	.000064	.000768		.0032
100	.79	0.5702	57.02	10,000	.638	.816	.000676	.004516	.79	.816
98	.65	0.2724	26.70	9,604	.334	.681	.000961	.004421	.65	.681
96	.53	0.0532	5.11	9,216	.030	.517	.000169	.000676	.53	.517
94	.37	-0.2347	-22.06	8,836	-.274	.350	.000400	.001760	.37	.350
92	.20	-0.5951	-54.75	8,464	-.578	.208	.000064	.000384	.20	.208
90	.13	-0.7965	-71.68	8,100	-.882	.107	.000529	.005539		.0384
88	.04	-1.2379	-108.94	7,744	-1.186	.047	.000049	.001103		.1804
$\sum 864$		0.2677	62.14	83,184					.021767	1.3593

table, labelled p_0 , gives the proportion of times (in 100) that each weight was judged heavier than the standard weight.²⁵

A phi-gamma function may be fitted to the proportions in Table XIX, and the χ^2 test may then be applied (with a reservation to be specified later) to evaluate the goodness of fit. The phi-gamma function is basically the same as the equation for the normal ogive, which may be written

$$p = \int_{-\infty}^x \frac{1}{\sigma\sqrt{2\pi}} e^{-x^2/2\sigma^2} dx, \quad [13]$$

with p standing for proportion of area under the normal curve from $-\infty$ to x . As usual, x represents a deviation of the variate X from the mean of X . The substitution in equation [13] of the symbol h for the quantity $1/\sigma\sqrt{2}$ yields

$$p = \int_{-\infty}^x \frac{h}{\sqrt{\pi}} e^{-hx^2} dx. \quad [14]$$

In this expression, $x = X - L$, where L is the value of X corresponding to a proportion of .50. The constant h is an index of the steepness or "precision" of the ogive curve. The phi-gamma function, written directly from [14], takes the form

$$p = \int_{-\infty}^{\gamma} \frac{1}{\sqrt{\pi}} e^{-\gamma^2} d\gamma \quad [15]$$

where γ has been substituted for hx .

In fitting the phi-gamma function to a set of observed proportions, it is necessary to obtain estimates of the constants h and L . Since $\gamma = hx$ and $x = X - L$, it follows that

$$\gamma = hX - hL. \quad [16]$$

This equation is linear in γ and X . The principle of least-squares is invoked in deriving the following two equations with which estimates of h and L may be computed:

$$h = \frac{N \sum X\gamma - \sum \gamma \sum X}{N \sum X^2 - (\sum X)^2}, \quad [17]$$

$$L = \frac{\sum Xh - \sum \gamma}{Nh}. \quad [18]$$

In the derivation of these equations, the quantity that is minimized is

$$\sum (\gamma_0 - \gamma_i)^2 = \sum (\gamma - hX + hL)^2.$$

Before equations [17] and [18] can be solved, values of γ must be obtained. They may be found in a table of the phi-gamma function (that is, in a table based on solutions of equation [15] for values of p).²⁶ The table is entered with values of p_0 .

²⁵ The reason that the value of p_0 for the 100-gram weight is significantly greater than .50 is that a negative time error was operating. This means that there was a consistent tendency for the subject to over-estimate the weight of the second member of a pair. The tendency was present in all comparisons, but was most immediately apparent from the value of p_0 when the standard weight was compared with itself.

²⁶ Tables of the phi-gamma function (which give paired values of p and γ) are not usually included in statistics books published in the United States. However, it is easy

Values of γ corresponding to the nine values of p_o in Table XIX are given in the third column of the table. The fourth column gives values of $X\gamma$, the fifth column values of X^2 . The four sums needed for solving equations [17] and [18] appear at the bottom of the table. Estimates of constants h and L are computed as follows:

$$h = \frac{9(62.14) - (.2677)(864)}{9(83,184) - (864)^2} = 0.152;$$

$$L = \frac{(864)(.152) - (.2677)}{9(.152)} = 95.8.$$

The insertion of these estimates into equation [16] gives

$$\gamma_t = 0.152X - 14.562.$$

This equation was used to calculate the values of γ_t shown in Table XIX. The corresponding values of p_t were read from a table of the phi-gamma function.

The χ^2 test may be applied (with a reservation) to test the goodness of fit, that is, to test the hypothesis that the difference between the observed and theoretical proportions arose from sampling fluctuations and, consequently, that the observed proportions may be satisfactorily represented by the phi-gamma function. The procedure for calculating the value of χ^2 based on all nine theoretical proportions is illustrated in the two columns of Table XIX denoted by (A). Values of $(p_o - p_t)^2$ were first obtained and were then divided by the product $p_t q_t$. This single division by $p_t q_t$ took account of differences between p_o and p_t as well as differences between q_o and q_t (where, as usual, $q_o = 1 - p_o$ and $q_t = 1 - p_t$). It can easily be shown that

$$\frac{(p_o - p_t)^2}{p_t} + \frac{(q_o - q_t)^2}{q_t} = \frac{(p_o - p_t)^2}{p_t q_t}.$$

Emphasis is here given again to the fact that in the calculation of χ^2 , account must be taken of the frequency of occurrence *and* the frequency of non-occurrence of an event.

The sum of the second column under (A) is .021767. This is not the value of χ^2 . However, it becomes the value of χ^2 after it has been weighted by 100, the number of judgments upon which each value of p_o was based. In other words,

$$\chi^2 = n_k \sum \frac{(p_o - p_t)^2}{p_t q_t} = 100 (.021767) = 2.1767$$

where n_k is the number of judgments per proportion. The number of df is $7 = 9 - 2$. It is two less than the number of rows in the table.²⁷ The computed

to obtain values of γ indirectly from an ordinary table of the probability integral. Since $z = x/\sigma$; $\gamma = hx$, and $h = 1/\sigma\sqrt{2}$, it follows that $\gamma = .7071z$ and $z = 1.4142\gamma$.

²⁷ The number of degrees of freedom for a table such as XIX is readily determined by starting with the number of rows. Each row contributes 2 df before any restrictions are imposed. (This is because each row actually covers two pairs of proportions. The pair which is omitted in the formula is q_o and q_t .) The restriction that $(p_t + q_t) = 1.00$ is imposed on *each* row; so 1 df is lost for each. Further, 1 df is lost for each constant estimated

value of χ^2 falls near the 95% level of confidence; so the phi-gamma hypothesis cannot be rejected.

The applicability of the last statement rests upon the validity of applying the χ^2 test in the manner illustrated. There are two weaknesses in the application, one relating to the size of two of the theoretical frequencies and the other relating to the restrictions imposed during curve fitting. Before the frequencies are considered, a few comments will be made on the nature of the restrictions that were imposed, although it must be said that a meaningful explanation of restrictions is mathematically rather complex and lies beyond the scope of this paper. Whenever a curve is fitted to a set of empirical data, there are usually several acceptable methods of determining values for the constants to be inserted in the equation. It is relatively easy to show that some of these methods lead to values of " χ^2 ," as computed by the familiar formula, which tend to be consistently larger or smaller than the values obtained when other curve fitting methods are employed. Therefore, when a value of χ^2 is computed to test the goodness of fit of such functions as the phi-gamma function, the method used to estimate the constants becomes a matter of importance. The computed quantity is distributed as χ^2 only when the restrictions imposed during curve fitting are both *linear* and *homogeneous*. (The meaning of these terms is explained more fully in a later section.) In many instances of curve fitting, and the present one is a case in point, the restrictions that are imposed do not have the requisite properties. When this is true, the investigator must be especially cautious in interpreting his results.

Some consideration must now be given to the theoretical frequencies represented by the theoretical proportions in Table XIX. The proportion .960 in the first row corresponds to two theoretical frequencies: 96.0 and 4.0. These values are estimates of the number of judgments "heavier" and the number of judgments "lighter" that the subject should have made when comparing the 104-gram weight with the standard weight. The proportion .047 in the bottom row also corresponds to theoretical frequencies of 4.7 and 95.3. These frequencies indicate that, according to the fitted function, the subject should have said "heavier" 4.7 times and "lighter" 95.3 times when comparing the 88-gram weight with the standard. The two frequencies of 4.0 and 4.7 are too small to yield quantities distributed as χ^2 and should not have been used in the calculations. They were included in the illustration as a means of contrasting the correct with the incorrect procedure. The two extreme proportions should have been combined with adjacent proportions. Table XIX shows the results after the combining has been done. New values of p_o , based on .97 and .90, and also on .13 and .04, are shown in the column labeled p_o under the general heading (B). Corresponding theoretical proportions are given in the column labeled p_i . The two combined values of p_o (.935 and .085) are averages based on 200 judgments each and the quantity $(p_o - p_i)^2 / p_i q_i$ calculated for each must be weighted by 200 instead of 100. Because of this differential weighting, it is better to multiply by n_b , as shown in the last column of the table, before the

from the data during the curve fitting process. Since two constants (h and L) must be estimated, the number of df is 2 less than the number of rows (or the number of pairs of p_o and p_i) that enter into the calculations.

TABLE XX
DATA FROM THE PITCH DISCRIMINATION STUDY BY STEVENS, MORGAN AND VOLKMANN (32)

X	JV		GS		RR		JM		DM		MJ*	
	p_s	p_t	p_s	p_t								
1.0	.00	—	.01	—	.01	—	.00	—	.00	—	(0.75)	.00
1.5	.00	—	.00	.036	.01	—	.00	—	.00	—	(1.00)	.00
2.0	.00	—	.00	.172	.01	—	.10	.088	.00	—	(1.25)	.13
2.5	.03	.044	.18	.464	.02	.093	.29	.273	.06	.089	(1.50)	.40
3.0	.18	.136	.45	.778	.21	.234	.49	.560	.30	.233	(1.75)	.647
3.5	.31	.310	.78	.947	.52	.450	.84	.816	.44	.457	(2.00)	.864
4.0	.49	.542	.95	1.00	.64	.682	.96	.950	.64	.695	(2.25)	.99
4.5	.74	.761	1.00	—	.84	.858	.98	—	.90	.870	(2.50)	1.00
5.0	.94	.905	1.00	—	.97	.953	1.00	—	1.00	—		
5.5	1.00	.972	—		.99	—			1.00	—		
6.0	1.00	—			1.00	—						
6.5												

* The frequency increments presented to subject MJ are given in parentheses at the left of the values of p_s in the last column. This subject was unusually acute and had to be tested with smaller increments than those used with the other five subjects.

sum of the column is taken. When this is done, the sum is the estimate of χ^2 . In other words,

$$\chi^2 = \sum n_i \frac{(p_0 - p_i)^2}{p_i q_i} = 1.3593.$$

The number of df is now 5, and the calculated value of χ^2 falls between the 98% and 99% levels. The phi-gamma hypothesis is still highly tenable.

This last calculation included two theoretical frequencies which are less than 10. They are 7.7 and 6.6. Further combining has not been carried out because such a step would, in effect, eliminate values of p which are critical in making a test of the phi-gamma function. There is perhaps some justification for retaining two theoretical frequencies as small as 7.7 and 6.6 when there are 12 other theoretical frequencies ranging from 18.4 to 93.4. An investigator may choose to be somewhat lenient in this regard, but leniency should never lead to the inclusion of frequencies of less than 5 or 6, and under no circumstances to a frequency as low as 0.5, one that is retained in an example in a widely used text (13, p. 181).

It is deemed advisable, except under unusual circumstances, to adhere to the policy of never using theoretical frequencies of less than 10. This means that if the χ^2 test is to be suitably applied in testing the phi-gamma hypothesis, the number of judgments at extreme values of the variable stimulus must be several hundred.

It is sometimes a temptation to pool the separate proportions obtained for several subjects in a psychophysical experiment. This practice should never be followed. The reason is clear; individual differences in judgments would yield interdependent proportions. It is permissible to apply the χ^2 test in relation to frequencies (or proportions) obtained from the judgments of a group of observers provided that each subject makes a single judgment. For example, a series of proportions for several different values of a variable stimulus, where no person makes more than a single judgment in the entire experiment, may be fitted with a phi-gamma function to obtain theoretical proportions; and the χ^2 test may be used to test the goodness of fit of the function. It is obvious, however that this situation is entirely different from one where the proportions are the averages of several sets of individual proportions.

Evaluation of an extended application of the χ^2 test to psychophysical data made by Stevens, Morgan and Volkmann. An interesting and instructive application of the χ^2 test of goodness of fit was made by Stevens, Morgan, and Volkmann (32) in their theoretical study of pitch and loudness discrimination. A representative segment of their data will be discussed. In one part of the investigation, they presented each of six subjects with an auditory stimulus which continued over a prolonged period but which was quickly changed in frequency, every three seconds, by a predetermined number of cycles. The duration of each altered segment was 0.3 sec. A subject was instructed to press a button every time a change in pitch was detected. The frequency increments that were used (in cycles per sec.) are listed in the first column of Table XX (except as explained in the note at the bottom of the table). Each subject made 100 judgments in relation to each of the increments which fell within his discrimination range. The observed proportions for the six subjects are shown in the table. (These proportions were furnished through the courtesy of Dr. Morgan.)

The study was designed to evaluate the theory of the neural quantum in auditory discrimination. The theory required, among other things, that the observed proportions for a subject could not be adequately represented by the phi-gamma function.²² Consequently, phi-gamma functions were fitted to each of the six sets of proportions in Table XX. All proportions greater than .97 and less than .03 were omitted during the curve fitting process. A few of these proportions later played a part in the calculations of χ^2 . This is one of the pitfalls to be carefully avoided. Another important aspect of the curve-fitting procedure was that the Müller-Urban weights were utilized. The nature of these weights and the detailed procedures for treating weighted data are adequately explained by Guilford (13) and need not be discussed here. It is enough to say that the weights are designed to diminish the influence of extreme proportions in the computations of the constants h and L .

TABLE XXI
VALUES OF h AND L SECURED BY STEVENS, MORGAN AND VOLKMANN (32)

Subject	h	L
JV	0.8520	3.9120
GS	1.2098	3.0525
RR	0.8476	4.1051
JM	1.0640	2.9001
DM	0.8754	3.5885
MJ	2.0432	1.6194

The values of h and L for the six fitted functions are shown in Table XXI. These constants were used in the manner illustrated in Table XIX to obtain theoretical proportions for each set of observed proportions. The theoretical proportions used by the investigators in calculating values of χ^2 are shown in Table XX. The six calculated values of χ^2 are given in the first row of Table

²² The theory also required that the proportions for the several subjects could be fitted satisfactorily with straight-line functions. The results for the straight-line fits will be omitted here. It is obvious, however, that a straight line can be fitted to each set of proportions in Table XX as a means of obtaining theoretical (calculated) proportions. The χ^2 test could then be applied to the differences between the observed and theoretical proportions, in a manner exactly analogous to that employed when theoretical proportions are obtained with phi-gamma functions. Whenever a comparison is to be made between the goodness of fit of two different functions, the same quantity should be minimized in the process of obtaining the constants for the functions. In their study, Stevens, Morgan, and Volkmann minimized the sum of squared differences between observed and theoretical values of gamma in fitting phi-gamma functions but minimized the sum of squared differences between observed and theoretical proportions in fitting straight-line functions. According to a theorem given by Cramér (5, p. 426 ff.), the values of χ^2 obtained with the phi-gamma functions would be expected to be larger than those obtained with the straight lines.

XXII, along with the number of df for each. (The other two rows of values in this table will be explained later.) Only one of the values in the first row (the value for subject *RR*) is sufficiently large to justify the rejection of the phi-gamma hypothesis at better than the 5% level of confidence. However, the six separate values of χ^2 are completely independent estimates and, therefore, may be summated to provide a single composite value of χ^2 , the number of df for which is the sum of the six separate df 's. The composite value appears in the

TABLE XXII
VALUES OF CHI-SQUARE BASED UPON THE DATA OF STEVENS, MORGAN
AND VOLKMANN (32)

Subjects							
	<i>JV</i>	<i>GS</i>	<i>RR</i>	<i>JM</i>	<i>DM</i>	<i>MJ</i>	<i>Composite</i>
χ^2	7.64	4.02	10.35	2.97	5.90	2.92	33.80
<i>df</i>	5	3	4	3	3	3	21
χ^2	4.94	0.13	4.06	2.97	5.90	1.02	19.02
<i>df</i>	4	2	3	3	3	2	17
χ^2	1.70	0.13	3.14	2.87	2.90	1.02	11.76
<i>df</i>	2	1	2	1	2	2	10

TABLE XXIII
SHOWING THE EFFECTS UPON CHI-SQUARE VALUES OF USING OBSERVED PROPORTIONS
WHICH WERE EXCLUDED IN THE DERIVATION OF THEORETICAL
PROPORTIONS (DATA FROM TABLE XX)

<i>X</i>	p_o	p_t	$(p_o - p_t)^2$	(A)		(B)		(C)	
				$\frac{(p_o - p_t)^2}{p_t q_t}$	$\frac{(p_o - p_t)^2}{p_t q_t}$	p_o	p_t	$n_t \frac{(p_o - p_t)^2}{p_t q_t}$	
3.0	(.02)	.093	.005329	.0629	—	—	—	—	—
3.5	.21	.234	.000576	.0032	.0032	.21	.234	.32	
4.0	.52	.450	.004900	.0201	.0201	.52	.450	2.01	
4.5	.64	.682	.001764	.0081	.0081	.64	.682	.81	
5.0	.84	.858	.000324	.0027	.0027				
5.5	.97	.953	.000289	.0065	.0065	.905	.905	.00	
				.1035	.0406			3.14	

From Column A: $\chi^2 = 100 (.1035) = 10.35$, with 4 df

From Column B: $\chi^2 = 100 (.0406) = 4.06$, with 3 df

From Column C: $\chi^2 = 3.14$, with 2 df .

last column of the table and is 33.80, with 21 *df*. This value falls at about the 3% level of confidence and indicates that the fits of the phi-gamma curves were generally poor.

A serious flaw in this application was that four of the extreme observed proportions, no one of which played any part in determining constants *h* and *L*, were included during the calculations of the values of χ^2 . A specific example will clarify the point. The observed and theoretical proportions for subject *RR* are listed in the second and third columns of Table XXIII. The observed proportion .02, shown in parentheses, was purposely excluded by the investigators when making least-squares solutions for *h* and *L*. Once obtained, these constants were crucial in determining the theoretical proportions. The investigators decided to include all observed proportions whose corresponding theoretical proportions were neither greater than .97 nor less than .03. This meant including the observed proportion .02 in the calculations for Table XXIII. It also meant the inclusion of observed proportions 1.00, .00, and .99 in the calculations for subjects *JV*, *GS*, and *MJ*, respectively.

As shown in Table XXIII, the estimated value of χ^2 was 10.35 when $p_o = .02$ and $p_i = .093$ were included in the calculation. If this pair of proportions is not included, the estimated value is 4.06. The number of *df* for the first estimate is 4, for the second, 3. There can be no doubt that the second estimate is the better of the two. In the calculation of χ^2 , it is incorrect to use observed proportions which have not been allowed to influence the magnitude of the theoretical proportions.

The values of χ^2 shown in the second row of Table XXII were obtained by excluding the observed proportions which played no role in the curve-fitting process. There are still six independent values, each with its own number of *df*. It is entirely legitimate to add these values to obtain the composite value of 19.02 (with 17 *df*) given in the last column of the table. This new composite value falls at about the 30% level and provides no basis for rejecting the phi-gamma hypothesis for the six sets of proportions.

Still another flaw in the calculations of the values in Table XXII was that theoretical proportions representing theoretical frequencies of less than 10 were not first combined with adjacent theoretical proportions. For example, the theoretical proportion .953 (in Table XXIII) represents theoretical frequencies of 95.3 and 4.7. A theoretical frequency of 4.7 is too small to yield a quantity distributed as χ^2 . Therefore, theoretical proportion .953 should have been combined, as shown in Table XXIII, with the theoretical proportion .858 to give $p_o = .905$. The modified value of χ^2 is 3.14, with 2 *df*. This value appears for subject *RR* in the third row of Table XXII. Similarly modified values for three other subjects are included in the same row. Again there are six independent estimates of χ^2 , and these may be added to obtain the single composite value of 11.76, listed in the last column of the table. With 10 *df*, this estimate falls around the 30% level.

It will be realized that the elimination of the small and large proportions makes impossible a really critical evaluation of the phi-gamma hypothesis through the use of the χ^2 test. But this is not a fault of χ^2 ; it is a weakness in the experimental data. Whenever χ^2 is to be employed, the experimenter must take precautions to insure theoretical frequencies of adequate size.

The principal pitfalls in the use of χ^2 with proportions have been designated.

Two others should be mentioned—the tendency to divide $(p_0 - p_t)^2$ by p_t alone instead of by the product $p_t q_t$ without including frequencies of non-occurrence, and the tendency to neglect to weight by the number of judgments upon which the proportions are based.

An application of χ^2 to percentages (Grant and Norris, 10). A recent application of the χ^2 test to percentages (or relative frequencies) is taken from a paper by Grant and Norris (10). These investigators were concerned with the influence of different amounts of dark-adaptation on the sensitization of the beta-response of the human eyelid to light.²⁹ One of their measures of degree of sensitization was the frequency of occurrence of the response. A subject looked straight ahead into a small box-like enclosure which was painted flat black inside and out. The stimulus was a small circle of light emitted from a circular milk-glass plate, 10 cm. in diameter, located at the back of the enclosure. When illuminated, the plate had a surface brightness of 241 millilamberts. The duration of the stimulus was about 750 milliseconds.

Four experimental conditions were employed, all subjects serving in each condition. The conditions differed in the amount of dark-adaptation present in the subjects. The amount of dark-adaptation depended upon the total length of time the subjects spent in darkness. The measure of amount of adaptation was the product It , where I was the surface brightness of the stimulus plate and t was the number of seconds spent in darkness. The It products for the four conditions were 28,920; 187,980; 347,040; and 506,100. The corresponding values of t were 120, 780, 1414, and 2100 seconds.

TABLE XXIV
DATA FROM THE EXPERIMENT OF GRANT AND NORRIS (10)

	Conditions			
	1	2	3	4
P_0	13.2	30.0	45.6	51.5
P_t	11.05	36.10	44.30	49.35

Thirty-three subjects participated in the experiment. A subject was first dark-adapted for 120 seconds. The stimulus light was then presented four times, with a "control" trial between the first two and last two presentations. The control trial served as a check on possible conditioning. The four stimulus trials were separated by dark intervals of 35 sec. on the average. The first four stimulus trials (coming after 120 secs. in darkness) were the trials for condition 1. Condition 2, in which the stimulus light was again presented four times along with a control trial, came after the subject had spent a total of 780 secs. in darkness. Conditions 3 and 4, with the stimuli presented in the same general fashion, came after 1414 and 2100 secs. in darkness.

It should be noted that each of the thirty-three subjects was given four stimulus trials in each of the four dark-adapted conditions. A count was made

²⁹ The beta-response is one of two reflexes displayed by the eyelid when the eye is light-stimulated.

of the number of beta-responses occurring in each subject in each condition. The results were then combined to provide frequencies of occurrence of the response for each condition. These frequencies were used in computing percentages. The four observed percentages are shown in the first row of Table XXIV. They indicate the *relative* frequency of occurrence of the beta-response in each of the experimental conditions. In condition 1, for example, 13.2% of the 132 possible responses of the eyelid displayed the beta-response. The other three percentages may be similarly interpreted.

A logarithmic function was fitted to the data. The two variables were It , the amount of dark-adaptation, and P_t , the relative frequency of occurrence of the beta-response. The fitted function was: $P_t = 13.38 \log_{10} It - 126.39$.³⁰ Its solution for the empirical values of It yielded the values of P_t given in the bottom row of Table XXIV.

A value of χ^2 was calculated as follows:

$$\chi^2 = \frac{(13.2 - 11.05)^2}{11.05} + \frac{(30.0 - 36.10)^2}{36.10} + \dots + \frac{(51.5 - 49.35)^2}{49.35} = 1.579.$$

Two constants were estimated from the data, leaving 2 df . With this number of df , the obtained value of χ^2 falls near the 40% level of confidence. This led the investigators to conclude that the data could be satisfactorily represented by a logarithmic function.

There are four mistakes in this application of the χ^2 test: two computational mistakes and two "theoretical" mistakes. The computational mistakes will be discussed first. The calculated value of χ^2 was not corrected to take account of the use of percentages instead of frequencies. It should have been multiplied by the ratio 132/100. The second computational mistake was a failure to take account of the frequency of non-occurrence of the beta-response in each of the four conditions. In condition 1, for example, the beta-response occurred 13.2% of the time and failed to occur 86.8% of the time. This latter percentage played no part in the calculation of χ^2 . All four percentages of non-occurrence should have been employed. The value of χ^2 for Table XXIV, when correctly computed, is 3.081. The number of df is still 2.

The other two mistakes were more basic. The use of χ^2 to test goodness of fit was not warranted, for two reasons. In the first place, there were linkages within conditions. Each of the subjects was given four trials in each of the four conditions of dark-adaptation. The results for the subjects were pooled. There were undoubtedly individual differences in capacity to display the beta-response; so there must have been linkages within conditions. In applying the χ^2 test, the investigators assumed, in effect, that the 132 trials per condition were given to 132 instead of 33 subjects. The test was inapplicable because the assumption could not justifiably be made.

The second reason that the χ^2 test should not have been used arises from the lack of independence from condition to condition. As already stated, a fundamental requirement for the use of the χ^2 test is independence between individual measures. The differences between individual subjects that manifested themselves in any condition were certain to be maintained in the other conditions; so it cannot be assumed that the values of P_t in Table XXIV are unrelated.

³⁰ With common logarithms, the function becomes:

$$P_t = 30.81 \log_{10} It - 126.39.$$

APPLICATIONS OF THE χ^2 TEST TO NON-FREQUENCY DATA

Some investigators are prone to use a χ^2 test of goodness of fit whenever a set of observed and theoretical values of any kind is available for comparison. This mistake apparently grows from a misinterpretation of the well-known formula for computing χ^2 :

$$\chi^2 = \sum \frac{(F_o - F_t)^2}{F_t} . \quad [10]$$

Because this equation involves differences between observed and theoretical values, the conclusion is reached that the summation of the weighted squares of differences between observed and theoretical quantities yields a meaningful estimate of χ^2 . Formulas superficially resembling equation [10] have been applied to non-frequency data, where theoretical values have been obtained from fitted curves.

Suppose that a study has been made of the amount of activity displayed by several groups of white rats deprived of food for differing lengths of time. Enough rats have been included in each deprivation group to provide means that are quite stable. These means, when plotted against time of food deprivation, show a systematic trend; so a curve is fitted to them. The equation for the curve permits the calculation of theoretical means which may be compared with the observed means. To test goodness of fit, differences between the observed and theoretical means are obtained. Each difference is squared and then divided by the appropriate theoretical mean. The sum of these weighted squared differences is taken as a meaningful estimate of χ^2 .

Two of the fourteen papers (4, 16) referred to in the opening paragraphs contain applications of this type. The fallaciousness of such applications becomes obvious when it is realized that values of χ^2 computed from non-frequency data vary in magnitude with the size of the units employed in measurement. Assume that two investigators have a common aim: To determine how the height of human males varies with age. They make measurements of the same individuals in various age groups, fit equations of the same form to their data, use these equations to compute theoretical values of height, and then obtain estimates of χ^2 in a manner similar to the one described above. One of the investigators has measured height in centimeters, the other in inches. Except for incidental discrepancies, the value of χ^2 calculated from the centimeter data will turn out to be 2.54 times the value calculated from the inch data.

The χ^2 test of linearity of regression. A χ^2 test is recommended in certain textbooks (26, p. 319; 12, p. 237) as suitable for use, with non-frequency data,

in evaluating linearity of regression. The formula, as usually presented, is as follows:

$$x^2 = \frac{\eta^2 - r^2}{1 - \eta^2} (N - k). \quad [19]$$

In this formula, η stands for correlation ratio; r for product-moment coefficient of correlation; N for the total number of measures; and k for the number of columns (groups) into which the measures have been divided. The number of df is $k - 2$. The formula yields a variable, the distribution of which approximates the χ^2 distribution, under certain conditions. It can be used with some degree of confidence provided that N is quite large and k is quite small, and provided also that the measures from column to column are independent and homoscedastic as well as normally distributed.

An exact test of linearity of regression, which is applicable whenever equation [19] is, can be made by computing an F -ratio. The formula is

$$F = \frac{(\eta^2 - r^2)(N - k)}{(1 - \eta^2)(k - 2)}. \quad [20]$$

The df 's for this F are $(k - 2)$ and $(N - k)$. The reason that the χ^2 test, as defined by equation [19], can be substituted under any circumstances for the F test, as defined by equation [20], is that the distribution of F approximates the distribution of χ^2 when one of the df 's for F is very small and the other is very large. The particular χ^2 distribution that is approximated is the one for the smaller df (that is, for $df = k - 2$). To state the point in another way: The sampling distribution of estimates of F , obtained with equation [20], approximates the sampling distribution of estimates of χ^2 obtained with equation [19], provided that k is very small relative to N .

Inasmuch as the χ^2 test, as represented by equation [19], is inexact, while the F test, as represented by equation [20], is exact, nothing is gained by using the χ^2 test.³¹

SPECIAL PROBLEMS: I. INDETERMINATE THEORETICAL FREQUENCIES

It sometimes happens, despite superficial indications to the contrary, that meaningful theoretical frequencies cannot be determined for a set of observed frequencies. This situation commonly arises from a lack of independence between measures. Two illustrations will be presented to reveal some of the chief sources of difficulty.

First illustration. The first illustration is concerned with coin-guessing data. Two hundred and forty university students each made a guess of heads or tails on four successive tosses of a coin. They recorded their guesses on individual record sheets. They were not told in advance the number of tosses that would be made, nor were they told how the coin turned up on the four tosses until the record sheets had been collected. The turns, in order of occurrence, were

³¹ A fuller explanation of equation [20], together with a detailed discussion of other F tests of goodness of fit, is given by Lewis (19).

H T T H. The succession of guesses of each student could easily be compared with this succession of turns and the number of correct guesses for each could be tabulated. As expected, the number of correct guesses ranged from none to 4. The results are summarized in the first two columns of Table XXV. As shown,

TABLE XXV
ANALYSIS OF COIN-GUESSING DATA

Number Correct	F_o	$P(n)$	F_t	$\frac{(F_o - F_t)^2}{F_t}$	$P(n)'$	F_t'	$\frac{(F_o - F_t')^2}{F_t'}$
0	15	.0625	15	.000	.0490	11.76	.893
1	47	.2500	60	2.817	.1680	40.32	1.107
2	60	.3750	90	10.000	.3129	75.10	3.036
3	86	.2500	60	11.267	.3289	78.93	.633
4	32	.0625	15	19.267	.1412	33.89	.105
Σ	240	1.0000	240	43.351	1.0000	240.00	5.774

15 students made no correct guesses; 47 made one correct guess; 60 made two correct guesses, etc. With frequency distributions of this type, it is common practice to apply the chi-square test after a binomial distribution function has been employed to calculate the required theoretical frequencies. The function for the present case, modeled after equation [3], would have the form

$$P(n) = \frac{4!}{n!(4-n)!} \left(\frac{1}{2}\right)^n \left(\frac{1}{2}\right)^{4-n} = \frac{4!}{n!(4-n)!} \left(\frac{1}{2}\right)^4, \quad [3']$$

and would yield theoretical probabilities of obtaining n correct guesses in 4.

Values of $P(n)$ calculated with equation [3'] are given in the third column of Table XXV with corresponding theoretical frequencies appearing in the fourth column. If these values of F_t were legitimate estimates, a value of χ^2 could be computed as illustrated in the fifth column of the table and used to test the hypothesis that the observed frequencies are distributed in accordance with equation [3']. In effect, this would be testing the hypothesis that each guess of every student was a purely chance occurrence, completely independent of every other guess, the probability of a guess of heads always being .50. The number of df is 4. If the hypothesis were true, the obtained value of χ^2 , 43.351, would not be expected to arise in random sampling once in a million times.

The use of a binomial distribution function in calculating theoretical probabilities cannot be justified in this case. There is good reason for believing that the guess of a student on any of the last three tosses was not independent of previous guesses. The binomial distribution function is applicable only when there is a sound basis for assuming that every event under consideration is completely independent of every other event.

Theoretical frequencies for the empirical data in Table XXV are indeterminate—except as they might be estimated from probabilities yielded by extraneous empirical data. When hypothetical probabilities do stem from

other empirical results, the hypothesis that can be tested may be quite different from the one that would be tested if a binomial distribution function yielded the theoretical values. For example, the data of Table XXV may be compared with somewhat similar findings published several years ago by Goodfellow (9), who analyzed the patterning of the guesses on five successive tosses of a coin by a large number of radio listeners. These listeners participated in the "telepathic experiments" conducted in 1937-38 by the Zenith Foundation. The coin was tossed in the broadcasting studio in Chicago. A total of 5,687 members of the radio audience wrote down their guesses in order and mailed in their answer sheets. They were told in advance that they were to make five guesses, but were not told until two or three weeks after the broadcast how the coin had actually turned up each time. The conditions of guessing were not identical with those holding when the data in Table XXV were secured, but were similar enough to permit a comparison of the results.

Goodfellow (9, Table II) tabulated the results on the radio listeners in a way closely approximating that used in Table VIII. In fact, the only difference of any consequence is that he tabulated percentages instead of frequencies. The "correct" pattern of *H T T H* was checked against the 32 patterns in Goodfellow's table to obtain the proportions of individuals in the total group of 5,687 that hypothetically made 0, 1, 2, 3, and 4 correct guesses. These proportions are given in Table XXV, in the column headed F'_i . The resulting value of χ^2 , as shown in the last column of the table, is 5.774 with $df = 4$. The hypothesis that may now be tested is that the patterning of the guesses by the 240 students was the same as the patterning of the first four of five guesses by the large group of Zenith Foundation listeners. The obtained value of χ^2 falls near the 20% level of confidence and provides no basis for rejecting this hypothesis. If the value of χ^2 had been large enough to justify a rejection of the hypothesis, it would not be possible to decide whether the patterning tendencies were basically different in the two groups or whether the differences in the conditions of guessing produced an apparent difference in patterning. Nevertheless, the test of the hypothesis, as stated, is an exact one. This is in sharp contrast to the indefiniteness which was present when a value of χ^2 was based on theoretical frequencies obtained with a binomial distribution function. Because of the strong likelihood of interdependence between the guesses of individual guessers, the highly significant value of χ^2 in the fifth column of Table XXV could be interpreted to mean that all guessers were biased, that some of the guessers were strongly biased while others were unbiased, that the probability of a guess of heads was not .50, that the probability of a guess of heads was .50 on some tosses but not on others, etc. The absence of independence and the consequent inability to obtain unequivocal theoretical (chance) frequencies made this application of the chi-square test a meaningless procedure.

Second illustration. Another illustration of the indeterminateness of theoretical frequencies comes from a paper by Seward, Dill and Holland (28). These investigators were concerned with an aspect of learning theory which need not be explained in order to describe their application of χ^2 . The experimental procedure was relatively simple. A subject sat at a table facing a panel. On the table in front of him was a row of twelve push buttons. Ten colored cards were used as stimuli. They were exposed, one at a time, in a small rectangular window in the panel.

In the "learning" series, the colors (except blue-green) were exposed once each in a predetermined order and the subject pressed the buttons until one was found which turned on a light. Nine of the ten colors were each paired with one of the buttons. The blue-green color was associated with two buttons, the fifth and eighth. The ninth button was a blank. The blue-green color was presented twice in the "learning" series. When first presented, the light was connected with the fifth button for half of the subjects and with the eighth button for the other half. On the second presentation, the connections were reversed. Thus each subject had an opportunity to develop associative connections between blue-green and button 5, and also between blue-green and button 8.

In the "test" series, the ten colors were again presented in a predetermined order and each subject was given six chances to push the correct button for each color. In this series, blue-green was exposed last and the connection was such that the pressing of button 5 or button 8 would turn on the light. The testing on any color was terminated as soon as the subject pushed the button that turned on the light or had pushed six buttons.

The investigators applied the chi-square test to the results on blue-green in an attempt to determine whether or not learning had occurred. Values of F_o and F_t , taken directly from their Table I (28, p. 231), are shown in the first two rows of Table XXVI. As seen, 24 of the 110 subjects pressed either button 5 or button 8 on the first trial. Twenty-five subjects pressed one of these two buttons on Trial 2, 22 on Trial 3, etc. Ten of the subjects failed to find either of the correct buttons in six trials.

TABLE XXVI

DATA FROM THE LEARNING EXPERIMENT OF SEWARD, DILL AND HOLLAND (28)

	Trials Required to Find Correct Button							Total
	1	2	3	4	5	6	>6	
F_o	24	25	22	21	4	4	10	110
F_t	18.3	16.7	15.0	13.3	11.7	10.0	25.0	110
p	2/12	2/11	2/10	2/9	2/8	2/7	?	*
n	110	91.7	75.0	60.0	46.7	35.0	25.0	

The values of F_t in the second row of Table XXVI were obtained by assuming that the probabilities of success on the six trials were those given in the third row. In other words, p was assumed to be 2/12 on trial 1, 2/11 on trial 2, 2/10 on trial 3, etc. A value of χ^2 was computed as follows:

$$\chi^2 = \frac{(24 - 18.3)^2}{18.3} + \frac{(25 - 16.7)^2}{16.7} + \dots + \frac{(10 - 25.0)^2}{25.0} = 31.3.$$

The only restriction imposed during the calculations was that $\sum F_o = \sum F_t = 110$. This left 6 degrees of freedom. With this number of df , the computed value of χ^2 is highly significant. It led the investigators to reject the hypothesis that the button pushing occurred by chance and to conclude that the subjects had, in fact, learned to associate blue-green with button 5 and/or button 8.

This application of the chi-square test was unwarranted because unequivocal theoretical frequencies for all trials, except possibly for trial 1, were indeterminate. The reason is that the events on trials 2 to 6 were related to previous events. The investigators seemed to be partially aware of this fact, as is shown by their choice of values of p . For example, consider the probability of success on trial 4; it was put at 2/9. This probability arose from the assumption that each subject remembered and avoided the three buttons that had already been pushed, completely forgetting, meanwhile, all choices that had previously been made when other colors were presented. Just prior to the blue-green test series, the subjects had each pushed up to six buttons in relation to each of the other nine colors. It is inconceivable that the subjects could have remembered perfectly the buttons that had been "used up" either during the blue-green series or before on other series, unless they adopted "systems" of pressing—a system, for example, of pressing from either end toward the center, or from the center toward either end, or from the third button toward the right, or from the tenth button toward the left, etc. The adoption of any system or systems of pressing by any subject would mean that any hypothesis concerning chance proportions was open to rejection prior to the calculation of a value of χ^2 .

When a decision concerning the legitimacy of theoretical probabilities is difficult to reach, it is sometimes helpful to see what happens if different probabilities are secured on the basis of new and not-unreasonable assumptions. It could be assumed, for example, that the 110 subjects used by Seward, Dill and Holland had, during the learning and early test series, discovered (on the average) that three of the twelve buttons (the two at the ends perhaps, and one in the middle) "belonged" to three colors other than blue-green, and thus by inference *did not belong* to blue-green. When blue-green was finally exposed,

TABLE XXVII
ALTERNATIVE ANALYSIS OF THE DATA OF SEWARD, DILL AND HOLLAND (28)

	Trials Taken to Find Correct Button						Total	
	1	2	3	4	5	6		
F_0	24	25	22	21	4	4	10	110
F_1	24.4	21.4	18.3	15.3	12.2	9.2	9.2	110
p	2/9	2/8	2/7	2/6	2/5	2/4	?	
n	110	85.6	64.2	45.9	30.6	18.4		

three of the buttons were already "used up" and were not likely to be pressed. The theoretical frequencies might be the ones given in the second row of Table XXVII. A value of χ^2 for this table is calculated as follows:

$$\chi^2 = \frac{(24 - 24.4)^2}{24.4} + \frac{(25 - 21.4)^2}{21.4} + \dots + \frac{(10 - 9.2)^2}{9.2} = 12.005.$$

With 6 df , this value falls slightly below the 5% level of confidence. It rests on dubious assumptions and, regardless of its confidence level, provides no grounds for meaningful conclusions; but the fact that it differs so greatly from the χ^2

value of 31.3 computed for Table XXVI strongly suggests that unequivocal theoretical frequencies are indeterminate in both cases.

It is not possible to provide rules-of-thumb for deciding whether theoretical frequencies are calculable or incalculable in particular situations. Decisions must ordinarily be based on careful logical analysis. However, *it is usually true that theoretical frequencies are incalculable if the observed frequencies are in any way related, and also if mutually contradictory assumptions can be made, with about equal justification, concerning the likelihood of occurrence or non-occurrence of the events (responses) that yielded the observed frequencies.*

SPECIAL PROBLEMS: II. THE NATURE OF IMPOSED RESTRICTIONS

All restrictions that are imposed during the determination of theoretical frequencies should be both linear and homogeneous. This limitation is seldom mentioned in either theoretical or practical treatments of the chi-square test, and even when mentioned is usually left unexplained. The main reason for the omission is that anything beyond a very superficial explanation cannot be given in other than mathematical terms.²² It follows that only a few very general ideas can profitably be included here.

It is probably quite obvious to most readers why restrictions must be imposed—why the sums of the observed and theoretical frequencies, for example, must always be equalized. The value of ΣF_o is fixed for any set of empirical data. The value of ΣF_t cannot “wander around any place” without at times yielding entirely impossible cell frequencies. Any hypothesis must be tied down at some point to the sample data. There should be as much freedom as possible (for example, as much freedom as possible for fluctuations in the individual cell frequencies) and yet there must be enough restrictions to bring the over-all values within the same general area.

The one restriction that must always hold may be symbolized as follows:

$$\sum F_o = \sum F_t, \quad [21]$$

or

$$\sum (F_o - F_t) = 0. \quad [21a]$$

²² The excellent attempt at elementary explanation offered by Greenhood (11, Chap. 3) is about as non-mathematical as it could conceivably be made, and yet requires a considerable amount of mathematical sophistication.

This restriction is clearly linear.

Some of the other restrictions that must be imposed in familiar applications of the chi-square test may be shown to be linear—and to be homogeneous also. Suppose, for example, that a normal curve is to be fitted to an array of observed frequencies. It is not enough to impose the single restriction specified by equation [21]. The reason is that a multitude of different combinations of values of F_t , all arising from normal distributions, will each, when summated, equal ΣF_o . A second restriction must obviously be placed. It may be written

$$\sum F_o X = \sum F_t X, \quad [22]$$

or

$$\sum X(F_o - F_t) = 0, \quad [22a]$$

where X is the measure associated with the cell. Stated in more familiar verbal terms, the restriction is that the means of the observed and hypothetical arrays of measures shall be equal.

There is still too much freedom for values of F_t , if a normal curve is being fitted. The "scatter" of the hypothetical measures must also be restricted. In symbols,

$$\sum F_o X^2 = \sum F_t X^2, \quad [23]$$

or

$$\sum X^2(F_o - F_t) = 0. \quad [23a]$$

These equations, in effect, state that the variances of the observed and hypothetical arrays shall be the same.

The three restrictions represented by equations [21], [22], and [23] are imposed when a normal curve is fitted to a set of observed frequencies.²² The equations are all linear and homogeneous in $(F_o - F_t)$. This is best seen from a comparison of equations [21a], [22a], and [23a].

Examples of non-linear restrictions. As a general rule, the restrictions imposed in applications of the chi-square test meet the linearity requirement. However, there are a few situations where non-linear restrictions are made. One of these has already been mentioned in the discussion of the phi-gamma function. In the phi-gamma case, theoretical frequencies are commonly obtained through a process that involves the minimizing of the sum of the squares of the differences between empirical and theoretical values of γ , where γ and p are non-linearly related. This non-linear relationship is clearly indicated by

²² One exception to this generalization should be mentioned. If values of the hypothetical mean and hypothetical variance are not estimated from the empirical data but come from some extraneous source, the only restriction is that $\sum F_o = \sum F_t$.

equation [15]. Despite the lack of linearity, the chi-square test is often applied. It must be realized that this test is not a rigid one; any estimate of χ^2 obtained from the differences between observed and theoretical values of ϕ is not necessarily distributed as χ^2 . Consequently, whenever χ^2 is used in evaluating the goodness of fit of the phi-gamma function, conclusions regarding the fit must be made with caution.

Another situation where a non-linear restriction is imposed in calculating theoretical proportions is in the use of Thurstone's Case V in treating data obtained by the method of paired comparisons (13, Chap. VII). The observed proportions are based on comparative judgments, the various stimuli being compared with each other. The proportions are translated into normal deviates (that is, into z -scores). Several z -scores typically enter into the calculation of each scale separation (distance). A scale separation based on several z -scores may be reconverted into a single "theoretical" z -score, the z -score then being used to obtain a theoretical proportion. A chi-square value computed from the differences between observed and theoretical proportions would not necessarily be distributed as χ^2 . Ordinarily, of course, the chi-square test is not applicable to paired comparisons data because of a lack of independence between the observed proportions.

Non-linear restrictions are always imposed in obtaining theoretical frequencies or proportions if the reduction (transformation) process is used in curve fitting. (Almost any method of estimating parameters for complex functions may involve non-linear restrictions.) To illustrate: Grant and Norris (10) fitted a logarithmic function to the percentages in Table XXIV. In doing this, they probably transformed values of the independent variable (that is, values of It) into their logarithmic equivalents and used these in estimating the parameters for the logarithmic function. If they followed such a procedure, the calculated theoretical percentages were dependent upon a non-linear restriction.

Unfortunately, there is no way of determining the exact influence of non-linear restrictions on estimated values of χ^2 . Therefore, if the chi-square test is applied, despite the imposition of non-linear restrictions, the investigator must be extremely cautious in interpreting the results,²⁴ bearing in mind that, as Cramér shows (5, Ch. 30), the calculated value of χ^2 is probably somewhat larger than it would be if the restrictions were linear.

CONCLUSIONS

Most readers will by now have correctly concluded that the chi-square test has a restricted usefulness. However, it usually cannot be replaced in those situations where it is applicable and it thus stands as a valuable research tool. Perhaps the chief trouble is that the test is too often applied without adequate prior planning; it is frequently "hit upon" and adopted after data have been collected and sometimes after other techniques of statistical analysis have been found unproductive.

²⁴ These statements hold also when the F statistic, based as it is on two independent estimates of χ^2 , is applied in testing goodness of fit. For example, several of the F -tests proposed by Lindquist (22) depend for their exactness on the type of restrictions placed in estimating the parameters of the fitted functions.

The aim of every investigator should be to plan, in advance, not only every detail of every experiment but every step in the analysis of the anticipated data. All contingencies cannot be foreseen; but if the chi-square test is to be employed, there is no good reason for failing to provide for independence among the measures and for frequencies of adequate size.

There should seldom if ever be any compromising on the requirement of independence.²⁶ There should usually be no compromising on the size of frequencies. There are occasions, of course, when it is very time-consuming and perhaps very expensive to add more cases to a mere handful. The best procedure under such circumstances is to try for an experimental design which utilizes each subject to the limit and leads to an analysis of data on an individual rather than a group basis. If it turns out that only a few subjects can be studied and the data on each one cannot be analyzed separately, it may still be possible to find a method of analysis which is more exact than the chi-square test. For example, if the data can be arranged into a 2×2 table and the individual cell frequencies are less than 10, the *exact treatment* proposed by Fisher (7, pp. 96-97) is to be preferred to the chi-square test. The treatment is rather tedious to apply, but in view of its exactness there is no adequate excuse for avoiding it.

Many users and would-be users of the chi-square test gain erroneous impressions from what they read about limitations on the size of theoretical frequencies. A textbook says that frequencies of less than 10 are to be avoided. This statement is often interpreted to mean not that 10 is a limiting value to be exceeded whenever possible, but that 10 is a value around which the various theoretical frequencies may fall; and if an occasional frequency happens to be as low as 4 or 5, that is all right because other frequencies will be larger than 10 and everything will average out in the end. A textbook that gives 5 as the suggested minimum tends to encourage the retention of impossibly small theoretical frequencies. And so does a text which states, in effect, that Yates' correction for continuity should be applied if the cell frequencies are 5 or less and precision is desired. This implies not only that frequencies of less than 5 are quite acceptable, but also that Yates' correction is an antidote for small frequencies. Both implications are fallacious.

The following excerpts from Yule and Kendall (35, p. 422) may help

²⁶ Any investigator who applies the chi-square test to interdependent frequency data should always feel obligated to include, in published accounts of his findings, a full explanation of the procedures employed and a justification of them.

to dispel false notions concerning the size of theoretical frequencies and also concerning the size of N :

In the first place, N must be reasonably large. . . . It is difficult to say exactly what constitutes largeness, but as an arbitrary figure we may say that N should be at least 50, however few the number of cells.

No theoretical cell frequency should be small. Here again it is hard to say what constitutes smallness, but 5 should be regarded as the very minimum, and 10 is better.

Hoel (14, p. 191), while giving 5 instead of 10 as the recommended minimal value of F_t , nevertheless emphasizes the importance of having a fairly large value of N by stating that if the number of cells or categories is less than 5, the individual theoretical frequencies should be larger than 5. Cramér (5, p. 420 f.) firmly recommends a minimal value of 10 and says that if the number of observations is so few that the theoretical frequencies, even after grouping, are not greater than 10, the chi-square test should not be applied. In all but one of the illustrations used by Cramér, the theoretical frequencies are considerably larger than 10, and in the exceptional case, he admits (p. 440) that the frequencies are smaller "than is usually advisable." An investigator handicaps himself whenever he applies the chi-square test in relation to small theoretical frequencies.

There are a few applications of the χ^2 test which have not been described and illustrated in the present survey, either because they are quite specialized in character or because they provide only approximate solutions. One of the specialized applications which may be of interest to some readers is Bartlett's test of the homogeneity of variance (3). Those who do not have access to Bartlett's original discussion will find a description of the test in Snedecor (31, pp. 249-251). Another specialized application of interest is the use of χ^2 in setting the confidence limits for a population variance from a known sample variance. The procedure is explained by Hoel (14, pp. 138-140) and need not be included here.

In general, any suggested applications of χ^2 which deviate from the well-established tests should be avoided except by those qualified to evaluate their full import or upon the advice of an expert.

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TASTE BLINDNESS TO PHENYL-THIO-CARBAMIDE AND RELATED COMPOUNDS

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I. THE PROBLEM

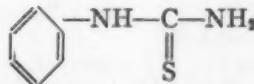
In 1931, Dr. A. L. Fox of the E. I. du Pont de Nemours Company was preparing a quantity of phenyl-thio-carbamide (hereafter referred to as P.T.C.) when a gust of wind blew some of the compound about. Dr. C. R. Noller, working in the same laboratory, complained of the bitter taste. Fox believed Noller to be mistaken, and put some of the crystals on his tongue. The taste sensation was neutral. Noller tasted some of the crystals and emphatically insisted that the compound was extremely bitter.

This incident touched off a series of experiments concerning the relationship of this and similar compounds to individual gustatory discrimination. Persons failing to taste the substance were termed "taste blind," following an analogous anomaly in color vision.

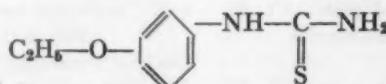
Fox (43, 44) reported that taste blind observers constitute about 40% of the Caucasian American population. This figure was confirmed in approximation by Blakeslee and Salmon (7), Snyder (37, 38), Fernberger (15), Blakeslee and Fox (6), and Parr (28). (See Table I.)

As it happened, the fact that some substances may taste differently to different people was not discovered by Fox, but by Laselle and Williams (22, 42) in 1926. These last investigators noted that creatine, a muscle constituent, was as tasteless as chalk to some people, and very bitter to others. They noted that one pound of lean meat may contain close to two grams of creatine, and it is probable that this would cause meats to have a distinctive flavor to the taster. This taste difference would be especially pronounced in soups made from lean meats which must contain considerable amounts of creatine.

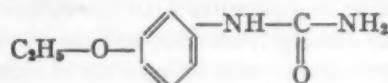
The structural formula of P.T.C., sometimes called phenyl-thio-urea, is given by



Para-ethoxy-phenyl-thio-carbamide is a substance closely related to P.T.C., and bears the same taste characteristics. Its structural formula is



In the above, if the single sulphur atom is replaced by an atom of oxygen, a commercial sweetening agent called "Dulcin" is obtained, whose structural formula is



Dulcin is used interchangeably with sugar in the manufacture of candy, with the difference that 250 times as much sugar as Dulcin is needed to achieve an equivalent sweetening effect. The similarity in chemical structure between P.T.C. and Dulcin was reported by Fox on several occasions (43, 44, 16, 17).

Indeed, it was this close relationship which encouraged Fox (17) to investigate the entire family of compounds to determine the chemical bases of taste. Elaborate experimental evidence shows that it is the CS linkage which determines the taste of the substance. Fox (16) reports taste differences for many related mono-aryl-thio-carbamides and symmetrical di-aryl-carbamides. Of these compounds, only two have been found which deviate from the class to which they belong, but Fox attributes this deviation to insolubility. Fox (43) and Riddell and Wybar (29) report similar taste differences for the entire thiourea group, and extensive work with di-phenyl-guanidine is reported by Snyder and Davidson (40). It is interesting to note that tetra-methyl-thio-carbamide is reported to have a sour taste to both tasters and non-tasters, while thio-carbamide itself is reputedly nauseating, neither bitter nor sour to both tasters and non-tasters.

Blakeslee and Fox (6) and Blakeslee (5) report that there are a certain few individuals for whom P.T.C. tastes sweet. Almost all investigators report a small portion of the population for whom P.T.C. tastes sour, salty, camphory, sulphury, etc. That these and similar reports are an error of experimental procedure was suggested by Blakeslee (14) and Blakeslee and Fox (6) who insisted that these could more accurately be described or classified as odors.

II. PHYSIOLOGICAL FACTORS

Threshold Data. Blakeslee and Salmon (8) have reported that P.T.C. has the widest range of thresholds of any substance studied. It has been clearly established (for several racial groups) that the distribution of tasters' thresholds forms a bi-modal distribution. This evidence is reported by Blakeslee and Salmon (7), Blakeslee (3), Blakeslee and Fox (6), Parr (28), Rikimaru (32, 33, 34), Setterfield, Schott and Snyder

(36), and Matson (26). As an illustration, we have plotted the data of Setterfield, Schott, and Snyder (36) in Fig. 1.

The individual threshold variability to P.T.C. is very large. Blakeslee and Salmon (8) disclose experiments with ten subjects who were examined four times a day during from four to nine days. Thresholds for a given individual were found to vary by a factor of from 2 to 256. Further experiments testing seven individuals at fifteen-minute intervals indi-

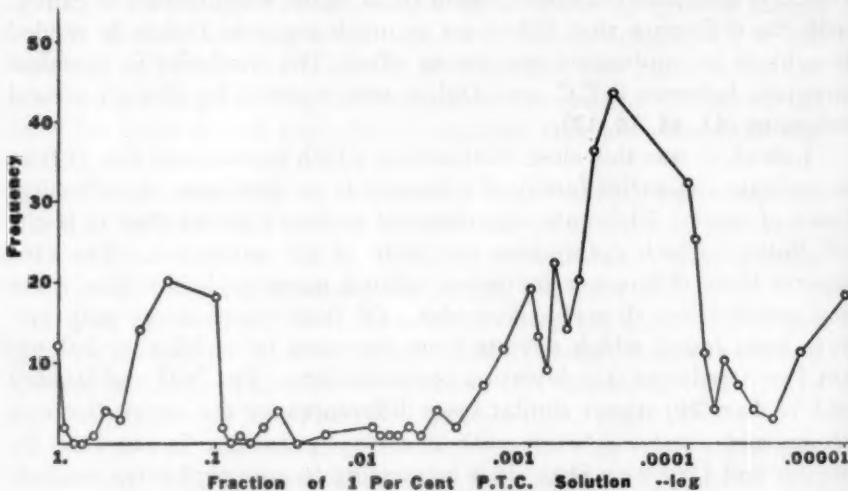


FIG. 1. Bimodal distribution of taste thresholds among tasters to P.T.C. The population is Caucasian. The number of individuals with a threshold of less than .00001 of 1% P.T.C. solution is 13. These data are plotted from tables given by Setterfield, Schott and Snyder (36).

cated that changes occur in this short time—and even in a time as short as five minutes. No regularity was found in either the direction or the amount of change, nor was there any rhythm in the changes.

Blakeslee (3) and Cook (14) note that threshold measurements for P.T.C. do not correlate with threshold measurements for quinine, picric acid, hydrochloric acid, saccharine, or salt.

Temperature in itself has no effect on the taste characteristics of P.T.C. When the solution is cold, however, less P.T.C. can be dissolved in it, and Blakeslee and Salmon (8) report a delayed reaction to such solutions. Analogously, Blakeslee (3) reports that many non-tasters may taste a hot solution, since more P.T.C. may be dissolved at high temperatures.

Saliva Condition. The saliva condition of the tasters and non-tasters

has been the subject of much experimentation. Blakeslee (2), Fox (17), Mee (27), Blakeslee and Salmon (8) have all suggested that it may be the saliva which is the determiner of the taste or non-taste and not the "taste apparatus" itself. Fox (17) specifically suggested the possibility that non-tasters have in their saliva a product, possibly a protein or a colloid, which precipitates this compound as a very insoluble substance which causes no taste sensation.

Snyder (37, 38) and Blakeslee (3) report that there is no correlation between the rhythmic change in the pH of the saliva and taste acuity. Blakeslee and Salmon (8) dried the tongues of each of their subjects with blotting paper and then rinsed these tongues with water. Four of six subjects were rendered less sensitive after this treatment, and the degree of desensitization varied considerably among the four testees.

The problem of the influence of saliva was analyzed by Cohen and Ogdon (13) in 1949. They reasoned that this question might be settled by allowing tasters and non-tasters to taste using *another* taster's or non-taster's saliva. Their results indicate that an individual will have the taste sensation when the following two necessary conditions are met: (1) he must have the correct "taste apparatus" and (2) he must have his own saliva. A non-taster cannot taste under any conditions, even when he uses the saliva of a taster. A taster cannot taste under any circumstances, except when he uses his *own* saliva; he cannot taste if he uses the saliva of another taster or non-taster. He can taste if he uses his own saliva, even though the saliva is placed on his tongue in exactly the same manner as the saliva from another individual. No observer can taste P.T.C. when the crystals are dissolved in water and no saliva is used at all.

By way of theoretical explanation, salivas are probably as different as fingerprints. Blakeslee and Salmon indicate that the saliva is important and Cohen and Ogdon note the fact that saliva is necessary to taste P.T.C. The observation that it must be the individual's own saliva may be brought about by the fact that the "taste apparatus" becomes, over the years, extremely sensitive and specialized to the particular saliva which the individual possesses. This being the case, when other saliva is introduced, it is equivalent to water, and no taste sensation results. Additional experiments by Cohen and Ogdon indicate that saliva *may* aid in the taste of other substances, but this effect is most pronounced with P.T.C.

Sex. The sex factor in the ability to taste P.T.C. has been investigated with conflicting results. Originally, Fox (43, 44) reported that taste deficiency was not related to sex, and this was confirmed by Snyder

(37, 38), Rikimaru (32, 33, 34), Setterfield, Schott and Snyder (36), and Snyder and Davidson (40). On the other hand, Fernberger (15) and Boyd and Boyd (11, 12) report the taste ability between the sexes to be significant. These last investigators published the ratio of female tasters to male tasters in Wales to be 1.66. The Fernberger ratio is only 1.11. Slight sex differences are noted in most other papers, but they are statistically insignificant.

Age. The possibility of age influencing the ability to taste P.T.C. has been investigated by Blakeslee and Salmon (7), Rikimaru (32, 33, 34), and Snyder (37, 38). On the whole, the age factor may be considered to be unrelated to taste ability. The apparent slight increase in acuity at certain ages, reported by Rikimaru, is probably insignificant.

Smoking. Hall and Blakeslee (20) report extensive experiments concerning the effect of smoking on ability to taste P.T.C. These experimenters set down the following conclusions: (1) Smoking has a definite effect upon taste acuity. (2) The time which must elapse after smoking before the threshold returns to a resting position varies with the individual. In these experiments only 58% returned to normal within one hour. (3) The incipient effect of smoking is the direct dulling of the "taste apparatus." (4) The true effect of nicotine on the nerves associated with taste appears to be the same as for other nerves—initial stimulation followed by depression.

III. GENETIC AND ENVIRONMENTAL FACTORS

The ability to taste P.T.C. is inherited as a simple unit-factor dominant in accordance with the primary Mendelian principle. This is an extremely important characteristic of P.T.C. since it affords the geneticist a simple test of the Mendelian laws.

Family Studies. In 1931 Blakeslee and Salmon (8) and Snyder (35) independently discovered the simple recessive character of taste blindness. They classified the parents of families into three groups according to their phenotypes—both parents tasters, one parent taster, neither parent taster. The taster vs. non-taster characteristics of the offspring were measured, and the Mendelian nature of the transmission inferred.

In the following year, Snyder (38) conducted a monumental study wherein he took data concerning 800 families at the Genetics Laboratory of the Ohio State University. His theory concerning the simple Mendelian inheritance was confirmed. Other confirmation using families has come from Rikimaru (32, 33, 34, 35), Strandskov (41) and Levit and Soboleva (25).

Twin Studies. Twenty pairs of twins, classified as identical by relatives, were studied by Rife (30). Each pair of twins had the same

taste characteristics with the exception of one pair of apparently monozygotic twins. But in this case the non-taster twin had suffered a general taste deficiency, which may have been the result of a severe case of poliomyelitis. Ardashnikov, Lichtenstein, Martynova, Soboleva and Postnikova (1) investigated 137 pairs of identical twins and found only three pairs who differed in their reactions to P.T.C. The non-taster sibling of one of these pairs was pathologically handicapped. However, no physiological or psychological anomaly was reported for the other two pairs who differed in reaction.

A somewhat more elaborate twin study has been conducted by Botsztejn (10) wherein he divided his population into three groups—tasters, taste weak, and taste blind. Using groups of identical and fraternal twins, he concluded that hereditary transmission followed a multiple allele series. Taster was dominant to taste weak, and taste weak was dominant to taste blind.

Transmission of Taste to Related Compounds. Snyder and Davidson (40) report that the inheritance of taste to di-phenyl-guanidine follows the same laws as P.T.C. Riddell and Wybar (29) conclude that taste to thiouracil is inherited in the same manner.

Linkage in Transmission. All investigators report that taste ability to P.T.C. is neither sex-linked nor sex influenced. Snyder, Baxter, and Knisley (39) have found that reaction to P.T.C., blood groups, and blood types are all inherited independently, nor is it related to brachydactyly (21).

Environmental Factors. Blakeslee (3) has suggested that taste to P.T.C. may be modified by environmental factors, but no evidence has been secured to confirm this.

IV. ETHNOLOGICAL FACTORS

The simple inheritance characteristics of taste to P.T.C. have encouraged anthropologists to attempt the tracing of ethnological groups by the per cent taster in various populations. It was hoped that the numbers would form rough racial clusters in the same manner as blood groups.

A summary of the extensive investigations by interested scientists is given in Table 1. Unfortunately, no ordering seems to result. Indeed the data themselves are internally inconsistent. Witness the range of American Caucasians from 60% to 82%. This indicates that the experimental procedures are weak, and some standard method of administration of P.T.C. should be agreed upon. Almost all of the studies in Table 1 merely describe populations and give results; there is little discussion of experimental technique.

TABLE I
PER CENT OF TASTERS TO P. T. C. IN VARIOUS POPULATIONS

Group	Per Cent Taster	N	Investigator
Caucasians (American)	60		Fox (43, 44)
Arabs (Syria)	63	400	Hudson and Peter (28)
Armenians (Syria)	68	294	Berberian (28)
Northern Jews (Palestine)	68	245	Yunovitch (28)
Semenites (Palestine)	68	59	Yunovitch (28)
Caucasians (American)	68	283	Blakeslee and Salmon (7)
Caucasians (American)	69	439	Parr (28)
Caucasians (American)	69	440	Snyder (37)
Caucasians (American)	70	3,643	Snyder (38)
Southern Jews (Palestine)	72	175	Yunovitch (28)
Caucasians (American)	73	1,025	Fernberger (15)
Egyptians	76	208	Hickman & Marcos (28)
Negroes (Alabama)	77	533	Howard & Campbell (28)
Caucasians (American)	80	232	Rife & Schonfeld (31)
Caucasians (American)	82	477	Setterfield, Schott, & Snyder (36)
Caucasians (Jews)	85	82	Rife & Schonfeld (31)
Formosans (Chinese origin)	89	5,933	Rikimaru (32, 33, 34)
U. S. Indians (mixed blood)	90	110	Levine and Anderson (24)
Negroes (American)	90	3,156	Lee (23)
Negroes (Kenya)	91	110	Lee (23)
Japanese	91	921	Fukoka (18)
Negroes (American)	92	107	Setterfield, Schott, & Snyder (36)
Japanese	93	8,824	Rikimaru (32, 33, 34)
Chinese	94	167	Chen and Chain (28)
U. S. Indians (full blood)	94	183	Levine & Anderson (24)
Formosans (Aborigines)	95	1,756	Rikimaru (32, 33, 34)
Egyptian Sudan natives	96	805	Lee (23)
Koreans	97	55	Fukuoka (18)

V. SUMMARY

This paper has discussed the taste blindness of certain individuals to phenyl-thio-carbamide and related compounds. Physiological, genetic, environmental, and ethnological factors have been considered.

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ARMY AIR FORCES AVIATION PSYCHOLOGY PROGRAM RESEARCH REPORTS¹

SPECIAL REVIEWS

FLANAGAN, JOHN C. *The aviation psychology research program in the Army Air Forces.* AAF Aviation Psychology Program Research Reports, Report No. 1. Washington, D. C.: U. S. Government Printing Office, 1948. Pp. xii + 316. \$2.00.

On July 15, 1941, John C. Flanagan was commissioned as a major in the Army's Officers' Reserve Corps and was at once assigned to duty in the Medical Division, Office, Chief of Air Corps. This was the beginning of the extensive research enterprise which was to become the Aviation Psychology Program of the Army Air Forces. Four years later the program included some 200 officers, 750 enlisted men, and 500 civilians. At that time, Colonel Flanagan could look back with pardonable pride upon a distinguished record of leadership in research planning, organization, and application. The comprehensive account of this work in the nineteen volumes of research reports under review in this issue of the PSYCHOLOGICAL BULLETIN symbolizes the effective manner in which the program developed under the direction of Colonel Flanagan and his many able associates. In historical perspective, this collective research effort will stand out as an important landmark in the evolution of applied psychology, as well as a very substantial contribution towards the effectiveness of the Army Air Forces in World War II.

In the first volume of the series, Flanagan presents a general account of the entire program, in three main parts: (1) a record of the expanding scope of the program, in terms of problems studied and of official directives on organization and functions; (2) a summary of the principal research findings and their applications to AAF problems; (3) an appraisal of the general contributions to theory, knowledge, and techniques of aviation psychology. The second section is a condensation of the results reported in the other eighteen volumes, which are reviewed individually in subsequent pages of this issue.

The official account of the expansion of the program is most impressive. In the beginning, research was restricted to the selection of pilots. Soon its scope was broadened to include bombardiers and navigators.

¹ *Editor's Note.* The nineteen volumes in this series were issued over a period of more than two years, the last report appearing in the spring of 1949. In spite of the long delay in the completion of the series, due partly to printing difficulties, it was decided to postpone the publication of any of the reviews until all of them were available. The advantages of simultaneous and somewhat coordinated evaluation of the entire program seemed to outweigh the possible decline of interest in these notable war-time research achievements with the passage of time.

Eventually gunners were added, and so the selection and classification of all air-crew personnel became the responsibility of the psychological research units. Attention then turned to the complex field of training, in which some of the most outstanding work in the program was done. In late 1943 interest in operational problems was developing, since the ultimate validation of selection, classification, and training procedures could only be found in combat performance. In May 1944 an evaluation and research detachment was flown to the European theatre to study combat operations. Permanent research units for combat commands were later authorized, but the war ended before they could go overseas. Finally, at the end of the war, the conception of an integrated research organization had crystallized which would study selection and classification, training, utilization of personnel, and the human factor in the design and operation of equipment. Although this plan, adopted in 1946, was to be largely supplanted two years later by a more comprehensive organization, these earlier recommendations of AAF psychologists laid the foundations for an integrated program of research and development in the Air Force.

In the third part of the book, Flanagan summarizes the general scientific and technological achievements of the AAF psychological research program. Like all other wartime research efforts, the gains were far greater in the technological than in the scientific dimension. One looks in vain for really significant scientific progress in any of the areas discussed; immediate military necessity would not permit long-range, basic research.

The classification research made possible extensive studies of the interrelations of traits, which seemed to support an atomistic theory of psychological organization. The factor analyses and the low correlations among apparently quite similar activities both indicated that "the number of basic uncorrelated aptitude factors . . . (is) probably much larger than most experts had supposed." In gunnery training, for example, men who did well on one type of trainer might do very poorly on another which seemed quite similar. Because of such results, generalization and transfer of training find little place in Flanagan's psychological scheme of things. (Except in the emphasis upon correlations between test scores and a variety of fairly dissimilar criteria!)

In the field of training the most outstanding contribution was the determined effort to secure adequate criteria of proficiency against which both classification and educational procedures could be checked. The nature and the determinants of effective behavior on the job became the central concern of the program in its later stages. These activities were for the most part models of skillful and persistent attack upon these most difficult of all problems in applied psychology. Although definitive solutions to these problems were not as a rule forthcoming, the methodological gains were very substantial. Future workers

in this field will know better how to approach such tasks, as well as what hypotheses and procedures to reject.

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DUBOIS, PHILLIP H. (ED.) *The classification program*. AAF Aviation Psychology Program Research Reports, Report No. 2. Washington, D. C.: Government Printing Office, 1947. Pp. xiv + 394. \$1.50.

DuBois' report is an important historical document. It is important only in part because much money was spent in the aviation psychology classification program—\$1,309,821 is the estimated expenditure for 1943 alone (p. 305)—some of which may be recovered from this book and its supporting files in a future war emergency. It is important because some of the mistakes made in experimental design can, if viewed critically and constructively, point the way to more adequate research development of differential selection tools. (The book being reviewed does not, however, look critically upon the experimental designs employed in the studies.) Furthermore, the report is important for the implications it has for organization and implementation of needed personnel research programs of large scope in the area of industrial as well as military personnel.

There are six chapters and a brief summary chapter. The first chapter deals with planning for selection of trainees and differential classification of pilots, bombardiers and navigators. The second chapter discusses the AAF psychological organizations concerned with selection and classification of air crew. The organization of the various AAF research units and the ten medical and psychological examining units are treated in considerable detail.

In Ch. 3 the classification batteries are discussed. The first battery to be used in the examining units of the Air Surgeon was developed early in 1942. Five changes were made in it during that year, and four changes were made in the next three years. During most of the period the Air-Crew Classification Test Battery included approximately 14 objective-type printed tests and six apparatus tests.¹

Of greatest interest to many personnel technicians is the manner of weighting the many tests comprising the classification battery:

The classification battery of February, 1942, employed an arbitrary weighting system: ". . . the necessity for an immediate classification demanded an a priori system utilizing the tests that were available" (p. 76). With reference to the classification battery of June, 1942: "On the basis of further professional analyses of the requirements of air crew, the weighting system was considerably revised" (p. 86). For the classification battery of August, 1942: "For the first time, the classification battery was used for selection instead of merely for

¹ *Editor's Note.* These tests are described in the reviews of Reports 4 and 5.

classification. . . . The weights were readjusted on the basis of professional judgment, since validity data were still incomplete and the test intercorrelations were not available" (p. 90, italics the reviewer's). But in the classification battery of July, 1943: "for the first time test weights for all three air crew aptitude scores were close approximations to multiple regression weights based directly on empirical data" (p. 99). (This weighting system suggests evolutionary progress.) The classification battery for September, 1944, included predictor scores for a total of seven air crew specialties: bombardier, navigator, bomb pilot, aerial gunner, air mechanic-gunner and radio operator-gunner. A final war-time revision of the battery was made in June of 1945, to segregate B-29 gunners from other types of gunnery trainees. The reviewer believes the following quotation suggests that the 1945 battery has atavistic traits: "The weightings for this specialty were based upon job analyses and one test, the *Pedestal Sight Manipulation Test*, with high face validity for gunnery, was introduced into the battery, carrying 20 per cent of the weight for the specialty" (p. 103). The systems of weighting for *differential prediction* were, for the most part, quite arbitrary. It is here that a new look at experimental design can perhaps be of greatest value.

Chapter four presents results of validity studies concerned with relation of total weighted scores, called stanines, and non-test data such as age and education to training criteria.² The training criteria against which these variates were correlated were pass-fail criteria. The fail criteria were eliminations from training for flying deficiency, for fear, and elimination at trainee's own request. The weighted predictor scores when correlated against such pass-fail criteria for many classes comprising thousands of trainees yielded biserial correlations for most samples of pilots that were fairly high and nearly always significant; those for navigators were moderately high and frequently significant; those for bombardiers were low and frequently not significant. Even moderately low biserial correlations yield striking predictive efficiency when the results are set up in charts, as is done often in this chapter.

One of the crucial indices of effectiveness of a differential classification battery (as contrasted to one designed for *mere selection*) would seem to be validity of a battery weighted for one specialty for another specialty. For example, how well does the pilot battery work for bombardier, the navigator battery for pilot, the bombardier battery for navigator, etc. Judging from the results presented the answer appears to be that the battery has higher validity for the specialty for which it was designed. However the critical observer may ask for: (1) an experimental design specifically aimed at evidence for or against the efficacy of a battery in differential prediction as contrasted to selection; and (2) safeguards against artificial contamination of the criterion. Did the trainee know his stanine? Did the instructor know the trainee's stanine? If so, did such knowledge influence the criteria of elimination for

² Validities of printed tests and of apparatus tests are not included in this book, they being given in Reports No. 4 and No. 5; validities of weighted tests when correlated with combat criteria are also presented in Report No. 17.

flying deficiency, for fear, or for elimination at the trainee's own request?

One table of information is presented in chapter four that is particularly damaging to the belief that stanines have considerable power in *differential prediction* as contrasted to mere selection. This information pertains to the validity of various stanines in predicting scores of trainees in pistol and carbine fire! For 350 cases the various air crew stanines were found to correlate from .25 to .40 when correlated with performance on the ground with the pistol, and from .21 to .31 with target performance with the carbine. The author refers to these results as evidence of "the versatility of the stanines" (p. 179).

Based on statistical and clinical approaches, Ch. 5 tells what happened to 1,311 air crew trainees who had been given tests but who were not screened by tests. For this experimental group the pilot stanine correlated .66 with the criterion. For the preliminary qualifying examination "only 45 out of 520 men who failed the examination were graduated from training, whereas the yield among those passing the examination was 211 out of 751" (p. 201). The study reported in this chapter is one of the most interesting to come out of wartime personnel research.

ROGER M. BELLows.

Wayne University.

THORNDIKE, ROBERT L. (ED.) *Research problems and techniques*. AAF Aviation Psychology Program Research Reports, Report No. 3. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. viii + 163. \$1.00.

A research program usually has two outcomes: (1) increased knowledge in the area being investigated and (2) increased understanding of the research methods used. It would have been a shame if the reporting of the large-scale AAF Aviation Psychology Program had been restricted to the first of these two outcomes. Fortunately, Report No. 3 was included in the series so that the psychological profession could share in the distillation of research experiences provided by that program.

Using the relatively unified aircrew selection and classification program as a frame of reference, Thorndike describes the practical research problems which arose and the adequacy of the methods of investigation applied. The report is organized around the major steps in a test validation program, separate chapters being devoted to job analysis, test development, criterion measurement, determination of test reliability and validity, and problems of combining and weighting tests when used in a battery. The influence upon test results of variables such as apparatus model, examiner, time of day, etc., is also discussed. After pointing out the problems common to both classification and training research, Thorndike adds a brief chapter on problems and techniques peculiar to

the latter, such as administrative difficulties, equating of groups, control of training variables, etc.

If Thorndike's style of writing had a lower Flesch count one might classify this report as a "How To" manual. It is this, to the extent that it provides an excellent guide to the step-by-step procedures required in the validation of an aptitude testing program. The sources of information concerning job requirements are presented, methods for test development are described, types of criteria are compared, and statistical methods and computational routines are presented in some detail. As such, it will serve as a useful reference in personnel psychology.

To consider this report as merely a manual of procedures, however, would be a grave injustice. For it performs the much more valuable function of taking the reader "behind the scenes" in order to see the *Why* as well as the *How*. Procedures are evaluated as well as described, limitations as well as strengths are pointed out, and theoretical implications as well as practical considerations are presented. Some of the topics of particular interest to this reviewer were the following: (1) the value of job analysis versus trait analysis, (2) objective versus subjective criteria, (3) selection versus classification, (4) evaluation of various measures of reliability in terms of the sources of variance affecting each of the measures, (5) problems of restriction of range of ability due to prior selection. These and other basic problems are treated systematically and the rationale as well as the methods of attack are described.

The editor recognizes (and clearly states in his Preface) one limitation of the report, i.e., its restriction in large part to the selection and classification program. Another limitation, in the reviewer's opinion, was the failure to discuss in detail the study of a non-preselected "experimental group" given aircrew training regardless of their test scores. Although this project is described in Report No. 2 of this series, it warranted more than the brief reference in this report in connection with the problem of restriction of range. From a methodological point of view, it represented a major step in providing crucial evidence on the validity of the classification program, and a discussion of its rationale, procedures, and general implications for experimental design would have been appropriate.

ALBERT S. THOMPSON.

Teachers College, Columbia University.

MELTON, ARTHUR W. (ED.) *Apparatus tests*. AAF Aviation Psychology Program Research Reports, Report No. 4. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. xxi+1056. \$3.50.

This report describes the largest program of psychomotor selection research yet undertaken. Although the National Research Council had conducted psychomotor selection projects on civilian flyers, little equip-

ment or information was available and many psychologists believed that it was not feasible to administer individual apparatus tests to large numbers of candidates.

The tests developed emphasized measures of precision at high speed rather than steadiness, equilibrium, or strength. The standard classification battery at various times included: (1) complex coordination (Mashburn type); (2) two-hand coordination; (3) two-hand pursuit; (4) discrimination reaction time; (5) finger dexterity; (6) arm, hand steadiness and aiming; (7) pedestal sight manipulation. Complex pursuit or continuous coordinations appeared to be most valid for flying skills. Major advances in procedure included standardized instructions, automatic cycling of trials, and multiple unit controls for testing four persons under each experimenter. Further improvements in the design of tests and the correlation of findings on specific tests with specific reasons for success or failure in flying skills would seem to call for more intensive empirical and theoretical job analyses.

In addition to the development and validation of tests for the selection of pilots, bombardiers, navigators, gunners, radar operators, etc., most tests were later devised to emphasize some variation in the performance such as working under distraction, stress or various degrees of difficulty, but with little increase in validity. Early test trials, including practice trials, were often as valid or even better than additional trials at later stages of learning. Rates of learning on various tests were quite specific.

This suggests to the reviewer that initial work methods adopted by a candidate in a new situation were important despite standardized instructions. The greater validity of more complex tests suggests as a second explanation that ability to attend simultaneously to various aspects of a complex situation might have been more important than the motor aspects of performance. Tests in which distractions were usually quite separate from the main operation would not be an adequate test of this theory. The success of work sample tests involving coordinated movements of hands and feet in a central task would be more relevant. Unfortunately, the most representative work sample tests were not employed for classification purposes until relatively late when there was no longer an opportunity for large scale validation, but results were promising.

In a program which frequently "washed out" 35 per cent of those who had already passed medical examinations, intelligence tests, and several years of college work and in which the average cost of flying training was on the order of \$25,000 per man, any reduction in "wash-outs" is important, particularly when psychomotor tests cost on the order of \$5.00 per person. Bar graphs showing the percentage of persons failing the primary training from each range of scores show clearly the savings from the use of single psychomotor tests. Multiple

correlations show that the battery of tests gave still greater validity. The saving of lives and equipment by elimination of trainees of lower aptitudes and the selection of abler trainees for actual combat duty are obvious advantages of a more strictly military nature.

To the reviewer this program has important implications as to needs in future research and training in psychology, e.g.: (1) The training of additional specialized personnel for *instrument design and construction*; (2) likewise, the relative neglect of *intensive job analysis and motion study* in psychomotor performances on military equipment calls for increased attention to this phase of training for future psychologists; (3) the further development of *more complete factorial batteries* of psychomotor tests would enable applied psychologists to select tests having the greatest probable validities for various skills; (4) the development of *procedures for setting up specific criteria* for success in acquiring each major type of skill; (5) the rapid mechanization of military aircraft and equipment, e.g., remote control aircraft and missiles, may outmode a great deal of present testing and require different aptitudes; (6) a *standby research and selection program, revised periodically*, is necessary to provide an adequate plan for emergencies. This in turn is likely to require an *administrative plan* which will encourage the widespread participation in research of civilian as well as military psychologists.

ROBERT H. SEASHORE.

Northwestern University.

GUILFORD, J. P. (ED.) *Printed classification tests*. AAF Aviation Psychology Program Research Reports, Report No. 5. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. xi+919.¹ \$3.00.

Printed Classification Tests gives a detailed account of methods, results, and conclusions concerning paper and pencil tests used and considered for use in the selection of Army Air Force pilots, navigators, and bombardiers. The volume will be of interest not only to aviation psychologists but to all psychometricians because of the important factor analyses reported. It presents impressive evidence of the effectiveness of the organization, direction and continuity of purpose of the group of psychologists who worked in the AAF research program.

The methods followed in the development of the AAF tests were in the approved text-book style of test development that is almost never pursued so thoroughly in actuality: job analyses were made; hypotheses were clearly formulated for factors that would appear to be needed; tests were given before the groups were selected; factor analyses were made of tests and criteria; new tests were based on the validity and factor studies; the process was repeated. This is the first successful attempt to use

¹ This review has been read and constructive criticisms provided by Mr. George Burgess and Miss Dorothy Everett.

factor analysis in a selection problem to show the factorial composition of the criterion as well as of the tests. The criterion unfortunately had to be only training school records rather than combat success. Factors extracted by the centroid method were rotated orthogonally to an approximate simple structure.

The tests tried were the commercial tests that looked most promising plus many new ones that were developed. Tests relating to these areas were examined: intellect and information; verbal ability; mathematics; reasoning; judgment, foresight and planning; integration; memory; visualization; mechanical; information; perception; size and distance estimation; spatial; orientation; set and attention; temperament and personality. In addition to tests the following kind of variables are correlated with training records: clinical type procedures, motivation, and biographical data.

Twenty-seven separate factors, rarely the ones expected, were identified with varying degrees of clarity. Seventeen factors have positive validity for predicting success in pilot training pass-fail criterion. These seventeen factors are in order of their validity for the pilot criterion: (1) Spatial Relation, (2) Mechanical Experience, (3) Pilot Interest, (4) Integration I, (5) Visualization, (6) Psychomotor Coordination, (7) Perceptual Speed, (8) Length Estimation, (9) Memory III, (10) Judgment, (11) Mathematical Background, (12) Integration II, (13) Visual Memory, (14) Planning, (15) Paired Associates Memory, (16) Reasoning II, and (17) Spatial II.

These factors range in estimated validity for the pilot training criterion from .32 for the Spatial Relations factor to .05. Since so many orthogonal factors are found in a single criterion it is to be expected that validities of the factors would be low. Furthermore the large number of cases used in these studies indicates that the validities reported are probably more dependable than some of the higher validities reported in industrial research studies.

In his chapter on "General Conclusion" the editor remarks that a complete classification battery should cover at least 25 factors. This probably overstates the case since the inclusion of those factors having lower validities does not contribute substantially to the validity of the composite. In addition, no more than 15 of the 27 factors are defined adequately enough for inclusion in a comprehensive classification battery.² The work that is reported is gigantic. There will be enough leads to explore for dozens of doctoral dissertations based on the factors found and suggested. Perhaps the most important new factor found is Judgment. Also of importance is that the nature of mechanical tests is clarified somewhat. Mechanical Experience comes out as a clear factor. Splitting what has been called Spatial Relations into a Visualization and three Space factors needs additional clarification.

Little comfort will be found in this volume for enthusiasts for the Rorschach, TAT, other clinical procedures for diagnosis, and personal-

² The reviewer is indebted to Dr. John Dailey for pointing out this fact.

ity inventories (including Bernreuter, Minnesota Multiphasic, and Humm-Wadsworth). Clinical procedures referred to are a clinical interview, observations of behavior during psychomotor testing, observations during a stress situation, observation during a short social situation. None of these procedures was found to be useful. Significant at the one per cent level were only the Thinking Introversion score of Guilford's Inventory with a validity coefficient of $-.09$ (judged not to be promising), Cooperativeness and Agreeableness of the Guilford-Martin Personnel Inventory, with validity coefficients of $.14$ and $.12$. Two categories on the Humm-Wadsworth, Epileptoid and Hysteroid, gave coefficients higher than those in the inventories mentioned above, $-.21$ and $-.18$, but they were significant only at the five per cent level since only 202 cases were used. Humm supplied case summaries which did not differentiate passes successfully from failures; however, his summaries were interpreted by the Air Forces' psychologists. Had combat criteria been available, it is possible that some of these personality measures would have been more useful.

The volume has shortcomings. The mass of material has not been smoothly presented. Clarity is exceeded by bulk. It appears to have been done hastily for the magnitude of the undertaking. The index is incomplete. Research psychometricians will not be seriously bothered by the shortcomings since the book will probably become a bible whose failings will probably come to take on sentimental value in the same way that philatelists treasure a rare stamp with some slight imperfection.

THOMAS W. HARRELL.

University of Illinois.

DAVIS, FREDERICK B. (ED.) *The AAF qualifying examination.* AAF Aviation Psychology Program Research Reports, Report No. 6. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. xvii + 266. \$1.25.

The AAF Qualifying Examination (originally termed the Aviation Cadet Qualifying Examination) was used as an initial hurdle in the selection of air-crew officers and (after June 1, 1944) in the selection of aerial gunners. The volume under review presents: a history of the examination (Chapter 1); the principles observed in constructing the examination (Chapter 2 and Appendix A); data on the validity, modification, and uses of the examination (Chs. 3 and 4); and studies of specific subtests considered for the examination (Chs. 5-10). Besides the Qualifying Examination, a few other examinations are very briefly reported on, viz.: the Aviation Cadet Educational Examination, the Flight Officer Examination, the English Expression Test, and the Victory Corps Aeronautics Aptitude Test (Chapter 11).

The volume is a meaty one, as evidenced by the 143 numbered

tables. No thoughtful reader can plow through this book in an odd afternoon; nor can any conscientious reviewer hope to do it justice within a few pages. In brief overview, what do we learn?

1. Paper-and-pencil tests can be extremely useful in a practical program of selection, even for so "non-academic" a function as flying a warplane. The Qualifying Examination was definitely useful in selecting men who later became successful flying officers. (Chs. 3 and 12.)
2. Research and experimentation led to a significantly improved battery of tests. (Chs. 2 and 3.)
3. Apparatus tests can significantly augment the validity of the paper-and-pencil tests. (Page 39.)
4. Within the limited scope of the reported experiments, paper-and-pencil tests designed to substitute for apparatus tests do not appear to fulfill their intended function. (Ch. 10.)
5. The theoretical possibility of using a "suppression variable" to improve the validity of a battery of tests was in this study carried as far as constructing and administering a special test for the purpose; but unfortunately validity correlations have not yet been obtained. (Ch. 5.)

Some more or less disturbing observations (about which the reviewer feels much less certain that the points above) may be worth mentioning:

1. Correlation coefficients tended to vary more than could be accounted for by random fluctuations of sampling; discouragingly, the variations were frequently in the "wrong" direction (i.e., the validity correlations turned out to be "lower . . . than substantial amounts of data had forecast . . ." (p. 39), or "the promising correlations obtained in earlier samples were not confirmed" (p. 187). A particularly interesting example of excessive variation is pointed out by the author on pp. 191-192; for other instances, see pp. 65, 70 (Table 5.10), 123, 156, and 245-252. The author does not undertake to explain or track down the possible causes of variation.

2. For one reason or another—almost certainly practical, and not due to oversight by the author—certain desirable data were either not collected or not analyzed. Thus, applicants for aviation-cadet training could take the Qualifying Examination repeatedly, "providing only that 30 days elapsed between each testing, and that different forms of the examination were used" (p. 1); yet no data are reported on practice-effects. Similarly, no data are reported on the correlation between successive forms of the Qualifying Examination. Rather often, in the case of a research sample, the answer-sheets containing the necessary basic data became unavailable before statistical analyses could be completed—so that while validity coefficients were computed for a given sample, reliability coefficients and intercorrelations (among the predictor-variables) had to be computed for a substitute-sample of imperfect comparability.¹ Another shortcoming is the rather frequent calculation of validity-data for the individual *parts* of a battery of tests, but not for the *total score* on the battery.

¹ Private inquiry indicates that, among various reasons for the unavailability of answer sheets, the principal one was the inaccessibility of data in storage, after transfer of personnel. After such a transfer it was apparently more feasible, on occasion, to collect new data than to obtain the original answer-sheets.

3. A capital lack, in the reviewer's judgment, is the failure to explain in detail whatever strengths or weaknesses the criterion may have held. (Not even an estimated reliability coefficient is offered.) The criterion generally employed is simply "graduation or elimination from elementary pilot training for reasons associated with proficiency in flying" (p. 2). Such a criterion seems extremely general or non-specific: not even the "academic" aspects of training are separated from the non-academic. Specific quantitative criteria would be more useful, not only for the validation and analysis of tests, but probably also as a means of stimulating better observation and evaluation by pilot-instructors. Granted that practical circumstances prevented the use of anything but a simple pass-fail criterion, some brief acknowledgment and warning of the limitations of the criterion would still have been in order.

4. Although the Qualifying Examination was validated against the *pilot* criterion, this same examination had to be passed also by prospective bombardiers, navigators, and (after June 1, 1944) aerial gunners. The administrative convenience of a single screening test is obvious; on the other hand, it seems likely that the various groups of flying personnel have differentiable aptitudes (see e.g., Table 11.2 on p. 220). (This, of course, is a criticism of an administrative practice, rather than of the present research report.)

The central practical question of this volume is: Was the AAF Qualifying Examination valid? The most explicit answer to this question is given in a study of over 1,000 "applicants for aviation-cadet training" who were entered into beginning pilot-training "regardless of their scores on the Qualifying Examination and the Air Crew Classification Tests" (p. 37). In this unselected sample, the validity of the Qualifying Examination (series AC12) was about .45-.50 (against the criterion of graduation from pilot training) (p. 38).

There can be little doubt that the last series of the Qualifying Examination (the AC14 series) was more valid than the first (the AC10 series). Because of the almost inevitable lack of comparability between the figures given for the first and last form, it is not possible to estimate exactly how much improvement in validity was accomplished; the reviewer's tentative judgment is about .20 point of correlation. This appears a remarkably large gain. Part of the gain may conceivably be due to improvement in the reliability or validity of the criterion; the reviewer has no information about this.

In conclusion, the reviewer would like to repeat what was stated at the outset: this book is indeed a solid, meaty work. The exigencies of wartime research have, to be sure, prevented as complete and close knit a series of studies as one might desire. But the author has striven conscientiously to present the information (both favorable and unfavorable) that has been gleaned. The task was immense, and the reviewer cannot close the book without a profound feeling of gratitude for a significant accomplishment. The volume makes contributions both of methods and results.

HERBERT S. CONRAD.

U. S. Office of Education.

GIBSON, JAMES J. (ED.) *Motion picture testing and research.* AAF Aviation Psychology Research Reports, Report No. 7. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. xi+267. \$1.50.

This volume is the official report of the Psychological Test Film Unit at Santa Ana Army Air Base, Santa Ana, Calif. Approximately half the report is concerned with the motion picture as a technique and is thus of rather specialized interest. Some of these studies are impressive in the care with which they have been carried out, and some of the results bearing on seating, illumination, and their like are clearly of use to certain readers. It is, however, the other half of the text that will interest most of the readers of the PSYCHOLOGICAL BULLETIN. This portion of the report may be divided into three subdivisions for discussion.

Aptitude tests. The Santa Ana Film Unit was engaged in part in the construction of selection and screening tests for a variety of Air Forces assignments. A number of ingenious motion picture tests for Flying Orientation, Ability to Judge Distance, Estimation of Velocity, Multiple Perception (dial reading) and other abilities were devised and are reported in some detail. The general reader will be struck by the very low correlations between these tests and other tests, between these tests and validating criteria, and even between these tests and themselves (various reliability criteria). Those who were engaged in similar tasks with the armed services will recognize, however, that r 's of .25-.55 may often represent genuine achievements. Nevertheless, the low level of predictability makes the permanent usefulness of these studies doubtful, and they must be accepted primarily as guideposts for others who will face such problems in the future.

Airplane recognition research. The investigations which Gibson's group carried out in this area are interesting for two reasons. First, they illustrate the danger of basing large scale activities on inadequate research. The "flash technique" of teaching aircraft recognition was accepted by both Army and Navy for reasons that were supported only by general theoretical considerations. The Santa Ana group has subjected the underlying assumptions of the method to a series of careful tests, only to discover that *none* of those assumptions could be verified in practice. Second, the findings have an intrinsic interest. We now know that training in making shape discriminations at very high speeds does not in itself increase the accuracy of such judgments when longer times are available. We also know that frank analysis of the airplane gestalt into certain component parts that distinguish it from others will aid, at least in the early stages of learning, in making the necessary discrimination.

General perceptual findings. The discussion of investigations related to the perception of aerial space and distance is undoubtedly the most important part of the book from the point of view of the general psychologist. Gibson and Glaser have presented in Chapter 9 a newly

organized "system" of explanation for depth perception. It differs in three main ways from traditional systems. First, more emphasis is placed on monocular stimuli. Second, these stimuli are all treated as matters of gradients of retinal stimulation. Third, the treatment of "gradients of velocity" for a moving observer is (so far as the reviewer knows) entirely original. All three of these departures from classical textbook treatments are suggestive and warrant the use of this chapter as supplementary reading for students of visual perception. At least two other contributions deserve mention. One is Gibson's emphasis on position on the vertical meridian as a factor in depth localization. Vertical position (the so-called "stage effect") is an important influence under many conditions of viewing but is often completely omitted from discussions of depth. The other is the quantitative study of size constancy under natural conditions of viewing. The presence of practically complete constancy up to nearly half a mile when only the texture of the terrain gives any distance cues may force a revision of some notions about depth perception based only on laboratory findings and theoretical considerations.

W. C. H. PRENTICE.

Swarthmore College.

MILLER, NEAL E. (ED.) *Psychological research on pilot training.* AAF Aviation Psychology Program Research Reports, Report No. 8. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. xix+488. \$1.75.

This volume summarizes research on pilot training accomplished cooperatively by psychologists in the AAF Training Command during World War II. The chief objective of Psychological Research Project (Pilot) was to develop objective measures of flying skill. Allied studies included a job analysis of the primary and specialized functions of student pilots, subjective measures of flying skill, verbal tests of flying information, as well as experiments which evaluate selected training procedures. The book consists of fifteen chapters in addition to six appendices which describe the more technical details of the data. The first fourteen chapters deal mainly with the methods and procedures employed in gathering and analyzing data obtained on large samples of Air Corps personnel under various conditions of flight-training, such as primary, basic and advanced levels, in single-, two-, and multi-engine planes, in contact and instrument flying. An excellent over-all summary of the project, together with recommendations for further work, is to be found in the concluding chapter.

It is scarcely surprising to find, in spite of the unusually skillful and imaginative approach of the investigators that progress in this new and complex area of psychology proved to be hard won. For the most part, coefficients of reliability and validity of the indices ran low, for reasons

of a sort already evident to most workers who have attempted psychological research under similar "field conditions." Lack of independent judgments from check-riders who determined the all-important pass-fail criterion, differences in standards from one training school to another (or even within different groups of check-riders at the same school), halo-effects as suggested by strong intercorrelations among different aspects of performance—these are only some of the factors contributing to the variance of the data. Further difficulties inherent in this area of research are emphasized by the finding that pilot performance could be measured with high reliability on the same day even with different observers, but that reliability coefficients tended to drop to zero when the same indices of performance were evaluated on another day by different observers. As the authors emphasize: "The airplane is a complex and variable piece of equipment and the air is an unstable laboratory"

Among the important accomplishments of this unit was the development of an 81-unit scale for measuring skill in instrument flying, which was sharpened to the point of fairly high reliability for this kind of work (test-retest reliability = .46). More significant still was an experimental study which determined under reasonably well-controlled conditions the extent to which additional training on a low-powered plane transfers to learning the technique of piloting a higher-powered plane. Within the particular circumstances of the training, transfer proved to be small enough to justify the conclusion that "in general 10 weeks of advanced two-engine training, all on the TB-25, was better than 15 weeks of training, 10 of which was on lower-power two-engine planes and only 5 on the TB-25." Again, on the basis of further experimental evidence, it was possible to conclude that, under the conditions of the investigation, "the type of plane in which students receive their basic training does affect their performance in advanced training." Beyond the unique value that such studies have in providing information critical to efficient training, they also demonstrate the feasibility of employing objective measures of flying skill for the quick evaluation of a large scale pilot-training program.

To the reviewer the most important single lesson in the report is the repeated demonstration of the limited value of many "testing" instruments—even when fully exploited, for doing a job that requires basic experimental research in order to obtain conclusive answers. As the authors point out, "While the results of laboratory experiments cannot predict exactly what will happen in a complex situation like that involved in flying training, they can tell one what to look for and how to go about setting up a series of training experiments to decide what kinds and amounts of training . . . will be most efficient in the long run."

J. L. FINAN.

Oberlin College.

KEMP, EDWARD H., AND JOHNSON, A. PEMBERTON. (EDS.) *Psychological research on bombardier training*. AAF Aviation Psychology Program Research Reports, Report No. 9. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. x+294. \$1.25.

Perhaps one's first and very general reaction to the military psychological research carried on during the past war is a certain feeling of pride that psychology had come of age and thus was able to make a unique and valuable contribution to the war effort. It is reassuring to learn that many of the concepts, principles, and even equipment which had been developed in the laboratory could be applied so readily to these military problems. To most of the publications within the Army Air Force series the writer reacted in that way.

Because so much money and time of professionally trained personnel was expended in these projects one also expects that they would have done more than apply what we knew, and would have made some contribution to psychological methodology, or to general principles. The contribution of the present study is one of emphasis, an emphasis upon the difficulty in obtaining a reliable criterion.

Superficially it would seem that the problem of obtaining an acceptable criterion for bombardier proficiency should not be difficult. The bombardier's military task is to drop a bomb on a particular target, and this task can be readily duplicated under non-combat conditions. By using a target with concentric rings the magnitude of error could be obtained either by estimations made by the pilot, a fellow bombardier, or by means of aerial photography. One could not quarrel with the validity of such a criterion, for it consisted of a fairly exact duplication of the combat job.

The major difficulty with this criterion, called the circular error, lay in the fact that it had zero reliability and hence could give no precise indication of any particular bombardier's proficiency. Obviously such a criterion had no correlation with any potential predictive measures. A number of studies were conducted to determine the sources of errors in the criterion measure.

In the first place the fault of the measure did not lie in the estimation of the magnitude of error for any single bomb burst. Photographic records could be read with accuracy and the estimates of observers on the magnitude of error were in agreement. A series of experiments indicated that the pilot, the degree of air turbulence, and the particular sight used were all factors contributing to the error. (If precision-built sights differ from each other, what about some of our home-made laboratory equipment?) An extensive experiment which served to control these sources of error was terminated before completion by V-J Day. Unfortunately it has not been continued.

However, the project was not so hampered by the criterion problem

that it was unable to make any contribution to bombardier training. It developed tests for selecting bombardier instructors, proficiency tests to measure knowledge of bombing and navigation, and was able to conduct a learning study which gave information on the amount of training necessary to reach a consistent level of performance in bombing accuracy. It was felt by the editors that the project indirectly influenced the bombardier training program through these and other activities so that the quality of training was higher than it otherwise would have been. Their beliefs were supported by a letter of commendation from the Commanding General of the AAF Central Flying Training Command.

DELOS D. WICKENS.

Ohio State University.

CARTER, LAUNOR F. (ED.) *Psychological research on navigator training.* AAF Aviation Psychology Program Research Reports, Report No. 10. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. ix + 186. \$1.25.

This book describes the research, development and services carried out by the Psychological Research Project (Navigator) under the Army Air Forces Psychology Program during World War II. The following kinds of research problems were conducted by the Project: (1) selection of navigator candidates, (2) navigator training, (3) development of objective ground measures of navigation skill, (4) development of aerial measures of navigation skill, (5) learning studies, (6) studies on instruction in navigator schools, (7) a study of the relative importance of variables involved in air navigation, (8) evaluation of training aids, (9) selection of navigation instructors, and (10) studies of combat conditions, post-combat adjustment, and group therapy. This impressive list of psychologically substantial and militarily useful problems is an indication that it is possible to conduct important research in conjunction with the action program of fighting a war. The navigator selection test battery, for example, had an average validity coefficient of .60 with a pass-fail criterion in advanced navigator school, which is about as well as our present college aptitude tests do in predicting college success.

One of the most interesting jobs described in the book is the development of proficiency tests, both on the ground and in the air, by means of which a more adequate criterion of performance than school grades alone could be obtained. A motion picture test, showing the actual terrain and other navigational information, was produced as part of this program.

The Project evaluated a 48-student training device which simulated on the ground the conditions of navigation in the air. An extensive experiment was designed to discover whether or not the trainer "trained."

As is the case with many training devices possessing high "face validity," the G-2 Trainer proved to have no special efficacy over equivalent time spent in the class room.

The principal new contribution to method in applied psychology is the use of factorial analysis and multiple correlation techniques in scientific job analysis. Error scores on eight and on 14 navigational variables were intercorrelated and two factor analyses were carried out. Both of these analyses identified three important factors. From these studies, it was possible to designate a particular operation (estimating compass deviation) as the primary factor involved in navigator error. This finding led to the recommendation for special training in the operation and suggestions for re-design of the instrument (astro-compass) to minimize the possibility for error. The interrelation of personnel selection, training and instrument design, and the dependence of these activities upon objective, reliable and valid performance measures is beautifully illustrated by these experiments.

The final chapter of the book is an evaluation of this two-year program. Although many problems remain unsolved or partially solved, it is concluded that the selection testing was successful and that the psychologist in the military setting contributed substantially to the solution of many problems not usually considered to be "psychological." Suggestions are also given for the operation of a peacetime research and development department of the Air Forces.

JOHN L. KENNEDY.

Tufts College.

HOBBS, NICHOLAS. (ED.) *Psychological research on flexible gunnery training.* AAF Aviation Psychology Program Research Reports, Report No. 11. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. viii+508. \$1.75.

This volume is a review and an evaluation of the work of the Army Air Forces Aviation Psychology Program in the area of flexible gunnery. Although the title of the book implies concern with problems of training only, the research of the flexible gunnery group embraced selection and equipment development problems as well. In fact, the research story told in the volume is convincing in demonstrating the "dog after its own tail" relation between the selection, training, and equipment aspects of psychological research in military or industrial work. The program on flexible gunnery began with a consideration of selection problems and the development of selection tests. The search for adequate criteria, however, against which to validate proposed tests of gunner selection, made it apparent that a thorough investigation of training procedures was necessary. Subsequent effort was directed toward the improvement of training techniques, the development of

achievement tests and tests of skill in gunnery maintenance, the evaluation of training devices, and the selection of gunnery instructors. Since these teaching problems were not unrelated to questions of more efficient ways to use available equipment, interest in operating methods developed, leading eventually to inquiries into possible improvements in equipment design through the modification of controls, sights, and other operational features. This elaboration of psychological research interests from selection to equipment problems had many direct parallels in the work of other research groups during the war.

The most important systematic problem discussed by Hobbs and his collaborating authors is that of establishing a satisfactory criterion of gunnery performance. No completely satisfactory criterion was established during the four years of research described in the volume. Combat data, which one might expect to be the best possible kind of criterion, were of limited validity, since records for a particular gunner depended not on his skill alone, but on that of other persons as well, both friend and foe. Various synthetic firing devices designed to train men in the operation of gunnery equipment were of limited "apparent validity" and had reliabilities in the region of .75. Ground firing tests with moving targets had reliabilities between .50 and .70. Air firing at towed targets or with frangible bullets had reliabilities which were generally below .50. In the absence of more satisfactory criteria, therefore, the motion picture record, taken with a gun camera mounted on the gunner's equipment, was accepted as the best check of proficiency. Unfortunately, the reliability of scores obtained from the gun camera films was seriously affected by the type of course flown by the attacking plane and by other temporary conditions. But for both training and criterion purposes, the gun camera was not surpassed by any other type of scoring device.

A concluding evaluation of the general significance of the gunnery research program is provided by the editor:

The full development . . . of a valuable performance testing technique might be considered important. The studies done on training devices are possibly distinctive by virtue of their comprehensiveness, but the devices themselves have such limited use that this work may best be considered for its wartime applications. Some of the studies on the use of sights may provide suggestions for further research on fundamental problems of visual perception. The training programs developed and evaluated may have long range value as illustrations of the effective application of principles which have long been recognized as sound. Nothing new has been added to knowledge of selection procedures, an adequate job simply having been done to meet an immediate need. Thus, it seems accurate to say that few of these specific findings of gunnery research are of enduring importance.

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COOK, STUART W. (ED.) *Psychological research on radar observer training*. AAF Aviation Psychology Program Research Reports, Report No. 12. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. iv + 340. \$1.75.

Research on the selection and training of radar operators was carried out by three groups, the NDRC Project, Sc-70, NS-146, the Aircrew Evaluation and Research Detachment No. 1 of the Eighth Air Force, and the Psychological Research Project (Radar) of the AAF Training Command. This book is primarily a summary of the work of the Radar Project.

Apart from having value as a record of wartime psychological contributions, the report will provide information for those interested in the development of selection tests and standardized proficiency tests, since it was in these two fields that the project concentrated its efforts.

Through the sections on proficiency testing, the extreme difficulties of administration are emphasized. The continuous change and development of equipment, the differing emphases and doctrines in different schools, and the lack of training personnel to provide standardized administration of the proficiency tests and checks, particularly those carried out under the complex conditions of actual flight, all worked against the establishment of the desired evaluation program based upon known reliability and validity.

A distinction is made throughout the report between the observable activities and the psychological factors involved in the radar observer's task. This distinction led, at the outset, to a job description, and to a parallel job analysis, the latter employing primarily factors "definitely or tentatively isolated in the Aviation Psychology Program" (p. 24).

The distinction between description and analysis is reflected in a classification of selection tests and, to a less extent, of proficiency tests, into two types: job analogy tests inspired by the job description, and factor tests suggested by the analysis into psychological components. Validation of the selection tests against proficiency measures indicated that both types must be used to get the best estimate of future performance in the training program. The editor of the report predicts that tests based on psychological analysis will gradually supersede those based on job description.

The primary aim of the project was to establish valid selection instruments. The research purpose of the proficiency testing program was regarded as that of providing criteria for validating the selection tests. In addition, performance on training bombing missions was subjected to a critical analysis, with a view to developing criterion measures. As in other selection research projects with similar aims, where the final task was some performance involving the operation of aircraft, it proved to be much simpler to state the aims and general methods of the valida-

tion procedure than to complete significant studies. The insurmountable variation in performance contributed by the complex factors in aircraft operation attenuated the reliability of the radar bombing error scores, just as it did in other attempts to obtain criteria of aerial performance.

The complexities of validating selection tests are thoroughly discussed in a penultimate chapter describing the results of two studies employing three batteries of selection tests validated against training course grades, against an air-crew classification "radar stanine," and, in part, against radar bombing error calculated from camera records obtained during aerial training flights. In spite of the besetting difficulties, a number of significant correlations were obtained. Since the results of these studies were available too late for application in the wartime selection program, their greatest value will lie in indicating to the uninitiated the practical problems which will be present in developing and evaluating tests of this sort.

A final chapter summarizes the principal points of the text and in addition discusses several areas in which training research was planned, including the fields of selecting and training instructors, and of organizing the training curriculum.

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DAILEY, J. T. (Ed.) *Psychological research on flight engineer training*. AAF Aviation Psychology Program Research Reports, Report No. 13. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. iii+227. \$1.25.

When the Pearl Harbor debacle gave rise to the immediate necessity of replacing heavy bombardment aircraft in the Pacific, the Army Air Forces decided, because civilian aircraft had handled the route successfully, to fly in the replacements from San Francisco. So many of the planes, however, crash landed at sea from fuel shortages that the Army Air Forces called in civilian companies for consultation. From their experience with long overseas flights, Pan American Airways had found it necessary to add a new crew member whose task it was to plan the flight from the standpoint of thorough knowledge of the mechanics of the craft. Fuel consumption, for example, was found to vary tremendously with the power setting, and one of the major duties of the new position, flight engineer, was to compute the optimal power setting.

It took Pan American two and one-half years to train a flight engineer, but the Air Forces in 1942 did not have two and one-half years. The designs of the B-29 had been on the drawing board since the beginning of the war and were to be in production in 1943, so the Air Forces set in a training program immediately at various schools in the south-

west. In 1944 some officers of the Psychological Research Unit No. 2 were assigned the task of preliminary investigation of the requirements and special characteristics desirable for B-29 crew members, and in 1945 the Psychological Research Project (Flight Engineer) was activated with specific directives for the selection and training of flight engineers.

The present report is a history (ch. 1) and description of the work of this project. It is notable for its account of work completed (chs. 4 & 5) in the face of what can only be called major difficulties. Consider, for example, the problem of establishing evaluative and selective criteria (ch. 2) for a new and complex position (1) when the student personnel varied from enlisted men with air mechanic experience to officer graduates of the Yale Technical Training Command Engineer's Officer Candidate School with B-29 training from the Boeing Factory School; (2) when the curriculum was under constant revision of load, content, and geographical locus; (3) when there were long delays and sometimes failures in procurement of mock B-29 equipment; and, finally, (4) the fact that urgent need for flight engineers resulted in extremely few failures and wash-backs, thus vitiating even the simple evaluative aid of grading systems. It is no wonder that many of the research criteria proved to be subjective; and the authors point out the need in this respect of long-time research studies. An appendix to the report includes copies of developed check lists, rating scales, and samples of items from the Flight Engineer Proficiency test.

The job analysis of flight engineer (ch. 3) indicated a responsible and key job in the airplane although the position in the command hierarchy was never specifically established. The flight engineer's duties involved the preparation of the flight plan, pre-flight craft inspection, starting procedures, proper operation of the power plant throughout flight, keeping flight log, post-flight inspection, and writing a post-flight summary report for the ground maintenance crew. His flight station was in the forward pressurized compartment and his instrument panel contained 36 dials, and a control stand fitted with 28 toggle-switches and 14 levers. He had primary authority in emergencies which endangered the loss of the plane.

The sixth and final chapter evaluates the research. Compromises between theory and practice resulted in only moderate successes, but valuable ones nevertheless for clearing the beginning ground of what will certainly be important research of the future. There is a straight-faced inclusion among the factors listed as influencing the trend of research: cessation of hostilities. The word "temporary" can almost be heard.

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WICKERT, FREDERICK. *Psychological research on problems of redistribution*. AAF Aviation Psychology Program Research Reports, Report No. 14. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. vii + 298. \$1.25.

This book is a competent and interesting account of the activities of certain American aviation psychologists during World War II. It should prove stimulating to academic psychologists and invaluable to applied psychologists concerned with similar or related problems in the armed forces and elsewhere. The volume contains eight chapters representing major research areas in which redistribution psychologists worked, as follows: (1) history and personnel of the redistribution psychological program; (2) the selection of instructors in the personnel distribution command; (3) combat leadership; (4) combat validation studies; (5) psychological studies of anxiety reaction to combat; (6) surveys of experiences of returned personnel; (7) attitudes and preferences of returned personnel; (8) evaluation of the redistribution psychological program.

The discussions and conclusions are generally distinguished by careful attention to the statistical significance of data based on samples whose basic characteristics including size, variability, manner of selection, and other important relevant factors are specified. Ample tabular material presented and referred to in readily apprehended fashion contribute greatly to the content and readability of the volume. Brief comments on the several types of studies will now be made.

Selection of instructors. To facilitate the further utilization of officer pilots, bombardiers and navigators and enlisted gunners who had been returned from combat duty, the development of test batteries was undertaken. They were designed to indicate general and special abilities, preferences, and other personal characteristics thought to be important for determining assignment to duty as: (1) instructors; (2) operating flying personnel in the Zone of Interior; (3) combat personnel overseas; (4) administrative and general ground personnel. Completion and use of such batteries was confined to the selection of instructors. Satisfactory validation against objective criteria of instructor proficiency was not accomplished during the period in which research was possible. Preliminary validation is not discussed in the volume being reviewed.

Combat leadership. Early studies provided descriptive, anecdotal delineation of behavior of successful and unsuccessful air combat leaders which was used to prepare rating scales. The latter were then employed to check the initial observations. Study of the interrelationships among overseas promotions and objective test and biographical data was a third approach. In all cases several independent samples of adequate size and appropriate characteristics were used to cross-check findings. "The successful combat leader (emerged) as an individual who exhibits outstanding proficiency in his specialty both as a flyer and as a tactical expert, shows consideration for his men, is consistent in his job performance, is sincere and self-sacrificing."

Combat validation studies. Due to the unavailability of useful records on combat performance, a data blank was developed for recording information obtained from the returnees themselves. Included among the items were missions flown, promotions while overseas, aircraft destroyed, highest decoration awarded, precombat flying accidents, flying accidents in combat, position flown, and average overseas efficiency rating. The limitations on the use of these criteria are discussed as are various controls applied as a result of experience gained in previous studies within the Aviation Psychology Program of the relationship between aircrew-candidate test scores and combat criteria. Eleven samples, covering two air forces, three test batteries and the positions of bombardier, navigator, first pilot, copilot and fighter pilot were studied. The N 's ranged from 85 to 1125. "To determine the degree to which stanines and test scores were associated with performance in combat as judged by the several criteria, each of the samples was successively dichotomized on the basis of each pertinent criterion, and the means of each of the stanines and test scores for the high and low criterion groups were determined. The significance of each of the differences between the corresponding means of the two groups was investigated by use of Fisher's t test." None of the differences reported was statistically significant, although it is stated that ". . . some of the results suggest the existence of significant relationships. . . ."

Psychological studies of anxiety reaction to combat. "Operational fatigue" or "anxiety reaction" characterized by tension, tachycardia, sweating, insomnia, nightmares, loss of appetite, irritability, inability to concentrate and low motivation was found by psychiatrists to occur in 5 to 10 per cent of flying officers and 15 to 20 per cent of flying enlisted personnel. No relationship between the aircrew candidate tests or the officer-instructor-selection tests, and anxiety reactions to combat could be found. It may be of interest that the Aiming Stress Test or Steadiness under Pressure, designed specifically to measure resistance to verbally induced emotional responses during a psychomotor performance, had no value for predicting anxiety reaction. The questionnaire known as the Personal Inventory consistently showed a biserial correlation of the order of .50 with psychiatric diagnosis. It should be clearly understood that this does not represent prediction of future behavior since the scores on the Personal Inventory were obtained at approximately the same time the psychiatric diagnosis was made though the two results are independent. The device is merely an aid to psychiatric screening.

Surveys of experiences of returned personnel. This chapter contains a large body of detailed data representing the experiences and impressions of several thousand men in respect to fear and courage in aerial combat, causes of mission-failures, and disorientation-while-flying. Limitations of space preclude any attempt to summarize. The findings contained herein should prove of considerable value to those responsible for training and instrument design.

Attitudes and preferences of returned personnel. As in the case of the preceding chapter, this one does not lend itself to ready summarization. One finding is of particular interest. "How the man reacts psychologically to combat determines his attitude (toward further combat service). The numerical count of hours, missions, and the like, does not." And yet, curiously enough, as stated in the previous chapter, "The hope of returning home after accomplishing a predetermined number of missions was claimed as an important factor in the control of fear. Seventy-five percent favored rotation after a defi-

nite number of missions, against 17 percent in favor of having each man fly until . . . he began to show signs of combat strain."

DONALD E. BAIER.

Personnel Research Section, A.G.O.

BIJOU, SIDNEY W. (ED.) *The psychological program in AAF convalescent hospitals.* AAF Aviation Psychology Program Research Reports, Report No. 15. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. viii + 256. \$1.25.

In September, 1944 the Army Air Forces Convalescent Hospitals were transferred to the Personnel Distribution Command and a Convalescent Branch of the Psychological Division of that Command was established with Captain Sidney W. Bijou as Officer-in-Charge. Psychologists in the Convalescent Hospitals had five principal functions: convalescent orientation, initial evaluation, progress evaluation, counseling, and research on the psychological problems associated with convalescent activities. At the peak of the program there were fifty officers and eighty-five enlisted men assigned to the psychological services branches of the twelve hospitals. Approximately one hundred and fifty thousand individual services were rendered to approximately thirty thousand convalescent patients. Almost half of the convalescent patients were classified as neuro-psychiatric, with *anxiety reaction* as the most common diagnosis. Typical patients were hospitalized for six to eight weeks exclusive of convalescent leaves and furloughs.

While the psychological programs in the various hospitals differed to some extent, psychologists normally participated first by undertaking a major share of the orientation discussion prior to each patient's transfer from the admitting ward to the convalescent hospital proper. Next, the psychologist conducted an initial evaluation of the patient on the basis of available records, inventories, tests, a case history interview, and a mental status interview. The psychologist then made recommendations to the personal physician, the most important of which had to do with the patient's daily five-hour program of activities. In addition to the services rendered to all incoming patients, psychologists were frequently asked to make special evaluations either to supplement the services of a psychiatrist or to provide personal, vocational, and educational guidance.

Psychological research in the convalescent hospitals was entirely oriented toward producing instruments for more effective services. Some of the instruments, such as the Rorschach, the Shipley Personal Inventory, the Wechsler-Bellevue Scale and the Cornell Selectee Index, were adapted from civilian forms. Others, such as attitude scales specific to the military situation, a biographical inventory and a test entitled Efficiency of Mental Application, were constructed on the spot.

Studies of validity were almost wholly dependent upon psychiatric

rather than behavioral criteria and reflect the well-known inadequacies of such criteria. Validity coefficients for the inventory type of instrument under these conditions ran quite high, e.g., a bi-serial r of .67 for the revised Shipley Personal Inventory.

Two exploratory studies of biographical information indicated the feasibility of using such items in the statistical prediction of a psychiatrist's estimate of a patient's prognosis. The authors are careful to point out, however, that since the psychiatrists used some of the same biographical information, the criterion is, from one point of view, contaminated.

The Rorschach Test and the Bender-Gestalt Test were studied in relation to the available criteria without conclusive results. This checks with other military studies of this type of instrument. It must be either that Dr. Rorschach's ink spots cast a peculiar magical spell over the psychological mind or that our system of statistical logic urgently needs some extensions. The Incomplete Sentences Test, by way of contrast, demonstrated substantial validity for differentiating groups of patients judged by psychologists to be psychologically unfit for duty ($r = .61$), although its correlation with the psychiatric criterion was considerably smaller ($r = .39$ and .41).

A method for quantifying the inter-personal behavior observed in group counseling was developed. Individual statements by patients and the group leader were classified under such headings as "displacement," "projection," "abreaction," "accepting," "reflecting," etc. Progress during a series of sessions was plotted with respect to the frequencies of the different types of responses.

The appendix contains the formats of several special instruments developed for use in the program.

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CRAWFORD, M. P. (ED.) *Psychological research on operational training in the Continental Air Forces*. AAF Aviation Psychology Program Research Reports, Report No. 16. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. vii + 367. \$1.50.

The avowed purpose of this volume is to "present a record of the research activities of aviation psychologists in the Continental Air Forces"; and ". . . to provide a summary of psychological research on operational training that will be of value to future research workers in this field." The first of these objectives has been accomplished in an admirable way. The style of the report is concise and clear. The organization is good, and there is little variation in general level of treatment from chapter to chapter—a fact which speaks well for the care with which the editing has been done.

Following an introductory chapter which treats the history, operating procedures, and organization of the Continental Air Forces as these were related to the mission of the research psychologists, there are nine chapters on individual air crew positions. The chapter headings are as follows: Fighter Pilot Studies, Photo-Reconnaissance Pilot, Airplane Commander, Copilot, Navigators, Bombardier, Flight Engineer, Radar Observer, and Flexible Gunnery. In each case, the results of studies designed to determine the relative validities of all classification tests for predicting the success in operational training of the various members of the aircrew are reported and evaluated.

The next three chapters concern the selection and evaluation of lead crews, studies in the acquisition and retention of aircrrew skill, and the results of a questionnaire which was designed to determine the attitude toward further duty of aircrrew personnel returned from combat. The final chapter is a summary and evaluation. In many ways this is the most valuable chapter in the book, for it is here that the editors present a critical evaluation of the work as a whole.

This reviewer does not intend to be captious when he states that the methods employed, the general type of result obtained, and the difficulties which were encountered are familiar to nearly all psychologists who were engaged in research on military problems. How well the second objective of this volume was accomplished depended in large part on forces over which the authors had no control. If military techniques, training methods and organization change suddenly and radically, the data reported will be of almost no value. On the other hand, the book does serve to acquaint research psychologists with what has already been achieved and need not be repeated. For this purpose, it has been well written.

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University of Wisconsin.

LEPLEY, W. M. (ED.) *Psychological research in the theatres of war*. AAF Aviation Psychology Program Research Reports, Report No. 17. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. vi+202. \$1.00.

A total of 16 officers and 31 enlisted men engaged in the research activities of the five principal projects with which this report is concerned. The primary missions were two-fold: to obtain first-hand information in the various theaters of war that would serve as combat criterion data against which to validate the original selection and classification tests; and to collect data on the requirements for the various tasks of airmen.

Secondarily, the missions were concerned with the development of proficiency tests for specialized personnel; the evolution of aptitude

tests not adequately covered by the original test battery, and such special problems as pilot fatigue in connection with long missions.

As for combat effectiveness, the criterion measures were selected from four categories:

1. Objective measures of combat proficiency, such as accidents, casualties and number of enemy planes shot down.
2. Records of administrative actions taken in combat units (e.g., transfer of inefficient personnel or their reassignment).
3. Ratings based on direct and systematic observation of combat effectiveness in specific missions or analysis of records thereof.
4. Ratings based upon general impressions, reports and incidental observations.

The investigators frankly state the various attenuating and biasing factors that affected the objectivity of their criteria. Included are halo effects in ratings, shrinkage of original populations, goodness of opportunity in determining kill scores of fighter pilots, atmospheric conditions, enemy opposition and deficiencies in record making and keeping, as illustrated by the heterogeneity (both inter-unit and intra-unit) of test batteries employed, scoring formulae applied, the time limits set and modes of recording scores of the selection and classification tests. Other inconveniences and frustrations of a pioneer attempt to fit the best man into each particular job under combat conditions are objectively described.

The report states that "the predictors used in the classification program were, in general, in some degree effective in the selection for combat" (p. 194). Of the 27 predictors for the bombardier category, 18 (66.7%) were predominantly positive. The 37 navigator predictors showed 16 or 43.2% that were positive for the most part. The fighter pilot category indicated 25 (58.1%) of the 43 items as positive as compared with 27 (61.4%) of the 44 predictor items in the bomber pilot category.

The fundamental inadequacy of selection and classification instruments employed is said to reside in their failure to predict the more complex and situational personality characteristics such as "emotional control or stability, judgment, maturity, leadership and motivation" (p. 194)—traits that "experts" judge high among the requirements for successful combat air crew personnel.

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University of Wichita.

DEEMER, WALTER L. (ED.) *Records, analysis, and test procedures.*
AAF Aviation Psychology Program Research Reports, Report No.
18. Washington, D. C.: U. S. Government Printing Office, 1947.
Pp. vii+621. \$2.25.

This volume is probably the least interesting reading and at the same time the most important of the entire series of AAF Aviation Psychol-

ogy Research Reports. It is composed of twenty-three chapters divided into three parts and five appendixes. Part I entitled "Test Procedures," ". . . . has a threefold purpose: first to give a brief account of the trial-and-error procedures that characterized early air-crew classification in the AAF Training Command and led to the adoption of standardized testing procedures; second, to present these standardized procedures in detail as an historical record of how standardization was maintained in air-crew classification in World War II; and third, to provide a basis for post-war testing procedures."

Part II, devoted to "Records Procedures," contains an excellent detailed historical description of the record keeping systems employed and should be useful to anyone desiring to use the records for research purposes in the future. One chapter (XIX) gives a detailed description of the procedures for "Obtaining Sums of Squares and Products with the IBM Alphabetical Accounting Machine."

Part III, "Analysis Procedures," is devoted mainly to enumerations and derivations of formulae for statistical analysis of data.

The five appendixes are reprints of various manuals for test administration, classification, and processing of candidates for air-crew. The volume is profusely illustrated with reproductions of record forms, punch cards, and pictures. This volume will be of inestimable value to researchers of the future who may wish to make special studies using data already available because of the minutely detailed descriptions of procedures and methods of record keeping.

E. R. HENRY.

Richardson, Bellows, Henry & Co.

FITTS, P. M. (ED.) *Psychological research on equipment design*. AAF Aviation Psychology Program Research Reports, Report No. 19. Washington, D. C.: U. S. Government Printing Office, 1947. Pp. xii+276. \$1.25.

The purpose of this volume is to review the research accomplished on equipment design and related problems of engineering psychology by the Army Air Forces. The work reported on was performed prior to 1 October 1946 at the Department of Psychology, AAF School of Aviation Medicine, and the Psychology Branch, Aero-Medical Laboratory, Engineering Division, Wright Field.

There are 21 chapters all together, 17 of which are reviews of separate research projects written by the individuals who originally carried out the investigations. The first chapter outlines and differentiates the field of engineering psychology, and presents the research areas, methods and techniques and research objectives of this branch of psychology. The second chapter is a survey of sensory display problems in the design of aviation equipment. Chapter 3 is a systematic analysis of human motor abilities related to equipment design and a suggested

program of research. The chapters which follow are detailed reports of particular investigations in one or both of these two areas.

Although the chapter reports which constitute the major part of this volume are oriented in terms of the practical problems of equipment design, they contribute, in many instances, to the general body of knowledge in experimental and applied psychology. These research reports may be classified under four different areas as follows:

1. *Visual display problems.* Specific projects are reviewed on the design of clock dials for reading in military time, a comparison of tables versus graphs as a means of presenting numerical data, the design of navigation plotters, the effects of dial diameter and angular separation of scale divisions on dial reading, and a comparison of cartesian-grid versus polar-grid presentation as they affect the interpretability of radar scopes.

2. *Tactual-kinesthetic display problems.* Studies are reported on the ability of pilots to reproduce pressures with the hands or feet, the tactual discrimination of knob shapes, shape coding of control knobs, and the location discrimination ability in the absence of visual cues.

3. *Sensori-motor interrelations.* There are reports on the control of aircraft roll as affected by different attitude indicators, the accuracy of sighting and triggering with different types of gunsight handgrip controls, two projects on the relation between control motions and the movements of visual indicators, the principles of control arrangement for sequential operations, and a comparison of different types of control movements in a compensatory pursuit task.

4. *Environmental effects on visual factors.* For example, efforts were made to study the effects of anoxia on visual illusions and the influence of positive *g* on the reading of aircraft instrument dials.

Although this book portrays the manner in which experimental psychologists can apply themselves and their methods to military problems, it is apparent that this type of engineering psychology has many civilian and industrial applications. The demonstration that a small change in an indicator or control system can produce a significant improvement in the efficiency of a machine operator is sure to appeal to industrialists. Aside from the practical applications, several novel and important contributions have been made to systematic psychology, particularly the results of studies on kinesthetic and tactual discrimination and the analysis of man's motor abilities. The association of psychologists with engineers has produced some remarkably good experimental apparatus. The manner in which practical military problems have yielded themselves to standard experimental methodology and statistical treatment is a tribute to the ingenuity of the investigators. Inasmuch as each experiment was originally an attack on a specific practical problem, there is occasional difficulty in introducing a particular chapter. On the whole, however, the editor has produced satisfactory continuity and an integrated picture of a new trend in psychology.

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BOOK REVIEWS

HILGARD, ERNEST R. *Theories of learning*. New York: Appleton-Century-Crofts, 1948. Pp. viii+409. \$3.75.

This is a "middle" book. It has the advantages and disadvantages of being in between.

It is half way between the undergraduate and graduate level. Thus it can be used profitably at either level, but must be supplemented at each. Although the writing is clear and simple, only the most advanced undergraduates will have enough background to achieve good understanding without carefully planned supplementary readings and lectures. At the same time, most teachers at the graduate level will want to go beyond this book with assignments from original materials and more detailed discussions. At both levels, the suggested readings at the end of each chapter will be helpful.

This book is also somewhere between a completely impartial survey and a polemic treatise that sets out to make a case against stimulus-response theories and for field-cognitive ones. The reviewer, who hereby warns the reader of his preference for stimulus-response concepts, will have more to say later about the direct and indirect ways in which the author's opinions influence his book.

Finally, it is a middle-sized book. The topics covered and the pages devoted to each are as follows: Introduction, 18; Thorndike, 32; Guthrie, 24; Hull, 40; Skinner, 30; Current Functionalism, 31; Gestalt, 32; Lewin, 25; Wheeler, 27; Tolman, 33; Theories Influenced by Field Conceptions, 31; A Point of View, 37; and References, 34.

In digesting the material in his extensive list of almost 600 references and condensing it to a mere 360 pages of text, the author has performed a difficult task which makes his well-indexed book a convenient and valuable source of titles. On the other hand, a truly rigorous analysis of the logic of learning theories with a critical evaluation of experiments is far too detailed and lengthy a task to be covered completely in the relatively few pages used. Within the wide range of material he considers, the author has had to be highly selective and touch relatively lightly on what he judges to be the high spots.

Quite naturally this necessary selection is influenced by the author's preference for field-cognitive concepts and reaction against stimulus-response ones. For example, he gives approximately 16.5 pages of text to Norman R. F. Maier and only 1.3 to Kenneth Spence. In describing Maier's results he says (p. 307): "These results are taken to be critical of quantitative theories such as those of Spence (1936) which reduce changes in behavior to modifications in the relative strengths of competing tendencies." But Spence's theory and the experiments supporting

it are not described. Similarly, with respect to the continuity-discontinuity controversy, the experiments by Lashley and Wade are described in considerable detail (somewhat tempered by a footnote pointing out that all of them have the same unbalanced feature and hence are uncontrolled), while those on the other side (e.g. by McCulloch and Pratt and by Spence) are omitted from the text.

Another type of selection is taking the space to elaborate field-cognitive types of theory beyond their author's published statements, showing how they might fill in gaps and deal with embarrassing experimental contradictions, but not doing this for stimulus-response theories. For example, pains are taken to develop what Koffka *might* have said if he had been more systematic about it, while stimulus-response is criticized for not handling punishment adequately; but there is no attempt to develop the relevant S-R concepts, no description of Mowrer's work on anxiety, and a serious misstatement (p. 62) of what Guthrie should predict.

Another way the author's opinion must be expressed is in deciding which concepts to examine critically and which to pass over uncritically. Here the author's critical examination of Thorndike contrasts with his much less critical acceptance of Skinner. Evidence against Thorndike's principles is presented in some detail; such fundamental questions as whether discriminated operants can be shown to follow different laws from conditioned respondents are not raised.

The choice of what material to include, to elaborate, to criticize is a legitimate matter of opinion. Attention has been directed to this only because its influence, though no less real, is much less obvious than that of direct argument. In fact, the introductory student has no way of detecting a bias of this kind.

The author's opinion also shows up occasionally in the choice of emotionally-toned words. For example, the hypothesis of movement-produced stimuli is referred to as an "ever-present alibi" while Wheeler's hypothesis that learning follows the laws of embryology and physical growth is described as an "audacious proposal." Some of the strongest opinions are expressed in informal editorial asides as in the statement (p. 144): "If the notion of operant behavior can be defended and extended, it may do much to break down the rigid stimulus-response conceptions which have too long been dominant in American psychology."

It is the reviewer's opinion that Guthrie does not get a fair hearing. To cite a single example, Guthrie's criticisms of opposing theories are characterized (p. 73): "When he attempts to show why they are not helpful he resorts to superficial criticisms and just occasionally to ridicule." The reviewer agrees that many of Guthrie's criticisms are written in a light and amusing style, but he does not consider them superficial. For example, when Guthrie says, "the rat is left buried in thought," he exposes the central weakness of Tolman's book as well as

of many other cognitive approaches, namely, that the theory deals with thought but has no means of getting it over into action. This is a weakness that Tolman has subsequently struggled to correct. An adequate criticism of the chapter on Guthrie is completely beyond the scope of a review; for more details the reader can turn to a discussion by F. D. Sheffield.¹

Though Hilgard shows his disapproval of the content of stimulus-response theory in a variety of ways, he definitely approves the rigorous logical form of Hull's theory construction and believes that learning science must ultimately be written quantitatively very much as Hull writes it. Though approving their content, Hilgard criticizes the loose form in which field-cognitive theories are expressed. He says (p. 259), "In order for the teachings of gestalt psychology to be assimilated to the ongoing body of scientific knowledge about learning, someone needs to do for gestalt psychology what Hull has been doing for behaviorism, that is, to state the laws or postulates more clearly and precisely, and then to design experiments which will true up these laws for the purposes of scientific prediction in terms of the detailed circumstances under which learning occurs. The suggestions by the gestalt psychologists have been sufficiently fertile that the carrying out of such a program of extension and validation holds rich promise."

It is interesting to compare this suggestion with one issued eighteen years before by Professor Hull: "A friendly invitation is extended to the Gestalt psychologists, and to such other schools as put forward distinctive theories of learning, to exhibit in similar detail a similar deduction from their own principles."²

Let us hope that this invitation will not have to be repeated after another eighteen years. When the two theories are put into equally good form, it will be much easier to judge their content.

This book is more than a superficial parade of theories. The careful reader will find that Hilgard has given a great deal of thought to many problems and will find much good common sense with which to agree. Hilgard makes an excellent case for the desirability of attempting to construct rigorous miniature systems and gives a clear description of the way several such systems may eventually be integrated to form a more comprehensive theory. He points out the usefulness of naturalistic observation in providing a broad foundation for theory and experiment, the importance of studying organization and perception, the higher mental processes and the socially-acquired hierarchy of human motives, the need for more attention to the problem of learning capacities, the advantages of young children as experimental subjects, and the importance of selecting the appropriate organism in studying neurological changes correlated with learning.

¹ Hilgard's Critique of Guthrie. *Psychol. Rev.* (In press.)

² *Psychol. Rev.*, 1930, 37, p. 253.

His own theory of "the provisional try" is stated so sketchily and so modestly subsumed under "A Point of View" that it is difficult to evaluate. He says (p. 337), "Such an interpretation takes all the mystery out of the backward action of effect." The reviewer believes that it substitutes some equally knotty problems. Before any predictions can be made on more than an intuitive basis (i.e. what would I as a reasonable being do if I were in that situation?), the author will have to state the laws governing such things as how a provisional behavior route is kept in suspension, how it has its provisional status changed by confirmation as an appropriate path of action and non-confirmation as an inappropriate one, how the memorial representation of what was done leads to action, and what determines the specific type of action that will be considered appropriate. Perhaps the author will attempt to attack these problems and formulate his own theory in more rigorous detail in a subsequent book.

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WOODWORTH, R. S. *Contemporary schools of psychology*. (Rev. Ed.) New York: Ronald Press, 1948. Pp. ix+279. \$3.00.

In this new edition, Woodworth is still Woodworth: in style, direct and informal, sometimes even sprightly; in substance, erudite, sane, critical but appreciative. Although no new schools have arisen since the first edition in 1931 (thank God, say all of us!) there have been important new developments in the existing schools, and these have led Woodworth to a rather drastic reorganization of his book. Indeed very few passages seem to have survived intact; and there is much fresh material and a fresh appraisal.

Both Structuralism and the Chicago variety of Functionalism, now moribund as distinct schools, receive reduced exposition, though Woodworth calls attention to their permanently valuable contributions. But he now distinguishes a more broadly conceived variety of functionalism with roots going back to the Greeks. (Titchener said it was merely commonsense, which for him meant that, though it might be valuable, it did not have the value of science.) This Functionalism starts "with the results accomplished by the individual and asks how they were accomplished" (p. 32). This is in fact the psychology of "Associationists, Old and New" including Thorndike and the successors of the Chicago Functionalists, of the later Behaviorists, and of nearly all applied psychology. Indeed the psychology of performance, as Woodworth elsewhere calls it, provides a broad platform upon which virtually all psychologists today can stand; and although he is too modest to make such a claim, he is one of the chief architects and builders of this platform.

Woodworth probably underestimates the historical significance of

early Behaviorism and the Meyer-Watson-Weiss revolution. Eclectic *au fond du cœur* that he is, Woodworth is not the man to appreciate a merely destructive force, which is just what he shows Behaviorism to have been. Almost nothing constructive, and at the same time distinctive, from that earlier period has survived, nothing, that is, that was not explicit in Functionalism. But it was the revolutionary ardor of Behaviorism which convicted the inheritors of Wundt of sterility; no introspectionist was ever quite the same after reading Watson.

The new Associationists and the new Behaviorists have developed theories rather than schools—Skinner's valiant positivism possibly excepted. Woodworth makes their place in the psychological scheme of things clear; but the graduate student preparing for his examinations must be prepared to go further.

Gestalt, Woodworth rightly sees as a reform of Structuralism; but it has steadily gravitated towards that "primary Functionalism" which is the common denominator of the contemporary scene. Concurrently it is winning almost universal acceptance, if not of Gestalt as a movement, at least of its principal concepts. Woodworth is at home with Gestalt, and most Gestalters will recognize that his treatment is a good introduction.

The analytical psychologists are less likely to be satisfied. Woodworth tries hard to extract the precious metal from the ore of psychoanalysis, but I think he is constitutionally unsympathetic. His chapter is therefore inadequate as an introduction to psychoanalysis. It is, on the other hand, a good statement of what the Freudian movement has so far contributed to psychology.

In his time, McDougall managed to knock together the heads of nearly all his opponents—that is to say, of nearly all other psychologists. "Such blandishments," Woodworth drily remarks, "failed to win votes." But McDougall's contributions to psychology cannot be judged from his manners in controversy. Woodworth shows that his influence and accomplishments were great; the very young men who have been taught by those who remember the twenties to scorn even the mention of McDougall are none the less more and more making motivation a foundation concept (McDougall spoke of purpose or *horme*), are more and more accepting the notion that motivation cannot, in G. Murphy's phrase, be pumped into an organism by social pressure as air is pumped into a football (McDougall insisted that the motives had an instinctive origin; as Woodworth points out, he does *not* say that our adult motives are instincts—that role he assigned to the sentiments). The psychology of the immediate future will not be McDougallian, yet it seems likely to resemble his doctrine more than that of any of his contemporaries except perhaps Freud.

Holism, formerly treated as part of Gestalt, now shares a chapter with Self-psychology. Again, I do not feel that Woodworth is sym-

pathetic; but if he does not see in these schools great values, neither does he see them as absurd. Instead, as always, he asks what corrective to our thinking is here, what do these intelligent men see that the rest of us are overlooking.

Thus the whole book is an exposition of that much misunderstood position, eclecticism. It is not mere syncretism, not a mere collection of a little that is good from this system or that. Eclecticism is itself a system, and the most intellectually arduous of all. In most systems the ego drive of the proponent is a part of the theory. The system-maker asks of any fact or concept, "Will it enhance my theory?" The eclectic asks, "Will it enrich my vision of truth?" Other systematists ask, "How can I get people to accept my views?" The eclectic asks, "What correction in my views must I make in the light of this evidence, these considerations?" The eclectic walks down "the middle of the road" because that is the best place from which to see what is going on every side.

This book may well be Woodworth's last testament; from it we may all catch a little glimpse of why Aristotle spoke of the truly great man as magnanimous—and of what he meant thereby.

HORACE B. ENGLISH.

Ohio State University.

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